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THE AMERICAN PSYCHOLOGIST

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In this Issue

Addresses by the Recipients of the 1958 APA Distinguished Scientific Contribution Awards:

Experimental Investigations of Species-Specific Behavior. FRANK A. BEACH	1
The Cognitive Activity of the Clinician. PAUL E. MEEHL	19
Pigeons in a Pelican. B. F. SKINNER	28
Internships for Doctoral Training in Clinical Psychology Approved by the American Psychological Association	38
New Members and Associates of the American Psychological Association	43
Comment	49
Psychology in Action	53
Psychology in the News	65
Psychology in the States	67
Notes and News	70
Convention Calendar	81
International Conventions	82
Call for Papers and Symposia: Sixty-Eighth Annual Convention of the American Psychological Association	83

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EXPERIMENTAL INVESTIGATIONS OF SPECIES-SPECIFIC BEHAVIOR

FRANK A. BEACH

University of California, Berkeley

At the annual meeting of Division 3 in 1950 I delivered a paper whimsically entitled "The Snark Was a Boojum" (Beach, 1950). In that lecture I presented a year-by-year analysis of articles published in the *Journal of Comparative and Physiological Psychology* and showed that there had been a steady decline in the number of species studied and in the kinds of behaviors investigated: more and more work being done with rats, and more and more experiments concentrating on learning. At that time I suggested that to develop a genuinely comparative psychology it would be necessary for investigators to study the behavior of many different species and to examine a much wider variety of behavior patterns.

I still regard these as worthy objectives, but in the ensuing nine years I have come to question the logical defensibility of *any* concept of a comparative psychology.

My doubts stem in part from the conviction that *Homo sapiens* is, in some ways, a truly unique species. This statement is not intended to be mystical. It does not demand a return to pre-Darwinian ideas about special creation. It is perhaps best documented by a quotation from the writings of one of our most eminent students of evolution, George Gaylord Simpson:

Man has certain basic diagnostic features which set him off most sharply from any other animal and which have involved other developments not only increasing this sharp distinction but also making it an absolute difference in kind and not only a relative difference of degree. In the basic diagnosis of *Homo sapiens* the most important features are probably interrelated factors of intelligence, flexibility, individualization, and socialization. All four of these are features that occur rather widely in the animal kingdom as progressive developments, and all define different, but related, sorts of evolutionary progress. In man all four are carried to a degree greater than in any other sort of animal (Simpson, 1949, p. 284).

Now, if man is a unique type of animal, this is exceedingly important, because many psychologists, and certainly the general public, think of psychology as a science of *human* behavior. I have ex-

amined many introductory texts, and this definition is clearly the dominant one.

This being the case, why is it that so many psychologists use lower animals as subjects in their experiments? Setting aside the obvious points concerning economy, methodological controls, short life span, etc., the fact is that nearly all so-called "animal psychologists" are primarily interested in, and draw their problems from, the area of *human* behavior. They use members of lower species as substitutes for human subjects.

A long time ago man dubbed himself the knowing or learning animal and thus indicated his conviction that a highly developed learning capacity is one hallmark of the human species. Therefore it is not surprising that most of the psychological experimentation on animals has dealt with learning and problem solving. These experiments take little or no account of the role played by learning in the normal life of the infrahuman species used as subjects. The general attitude seems to be that learning is learning, and it can be studied as well in one species as in another.

Not all psychologists share this implicit faith. Skepticism has been expressed by Hilgard. After listing the methodological advantages offered by animal research, Hilgard makes the following statement:

A price is always paid for the convenience of a given approach to a problem. The price to be paid for over much experimentation with animals is to neglect the fact that human subjects are brighter, are able to use language—and probably learn differently because of these advances over lower animals (Hilgard, 1948, p. 329).

These various considerations lead me to wonder whether it might not be desirable to explicitly restrict the concept of psychology solely to the study of human behavior. Since psychology is a frankly anthropocentric discipline, it is difficult to envisage what a *comparative* psychology would really be. Of course there is the term "animal psychology." But if this means merely "animal behavior," why not call it that?

It seems to me that a genuinely *comparative* science of behavior would not be oriented to any given species at the expense of others. It would not concentrate upon particular types of behavior to the exclusion of others. It would center its interests upon one type of organism after another and attempt to increase our understanding of behavior as it is shown by the species under consideration.

I am sure that I do not have to defend the proposition that the study of any kind of behavior in any species of animal is a scientifically respectable enterprise. Of course some degree of selection is essential, but the basis of selection is idiosyncratic and entirely at the discretion of the individual investigator. One worker may wish to concentrate upon grasshoppers, another on seagulls, and a third on human beings. However, if the goal is to build a *comparative science of behavior*, two desiderata seem obvious.

First the behavior selected for examination should be, so to speak, "natural" to the species. Insofar as possible it should be species-specific, a term I will explain shortly. Secondly, the kinds of behavior chosen for analysis should be as widely distributed as possible, phylogenetically speaking. This would increase the opportunity for interspecific comparison and improve the probability of arriving at valid and broad general principles or laws.

Let me repeat that what I am proposing differs from current practice in animal experimentation in that animals are not to be used as substitutes for people, and the kinds of problems investigated are not to be exclusively or even principally derived from human behavior or psychology. To make my point clear I must now tell you what I mean by species-specific behavior.

SPECIES-SPECIFIC BEHAVIOR

Species-specific behavior patterns constitute the normal behavioral repertoire. They are present in the same or very similar form in all or nearly all members of the species of the same age and sex. Many such specific patterns are characteristic of one and only one species, and Hebb has called them "species-predictable" (Hebb & Thompson, 1954).

Excellent examples of species-specific patterns are found in the songs of various birds. Experienced ornithologists can identify many species ex-

clusively on the basis of song. In other instances it is necessary to employ recording devices to detect species differences. Nevertheless it is exceedingly unlikely that any two species possess identical songs. Figure 1, taken from Thorpe (1956), shows tracings of sound spectrograph recordings of the songs of four closely related species of finches: the greenfinch, goldfinch, bullfinch, and hawfinch.

In interpreting slides like this psychologists are used to reading intensity on the abscissa and amplitude on the ordinate. This record is different. *Frequency* is shown on the vertical scale—the higher the mark, the higher the pitch. Along the horizontal scale is shown *time*. Each of these records represents a song lasting about 2.5 seconds. Different frequencies emitted at the same time reflect harmonies and other overtones. Reading from left to right, we see a record of 2.5 seconds of continuous song. The songs of the four species are obviously dissimilar.

Many fish construct nests in which the eggs are laid, and no two species make exactly the same kind of nest. Figure 2 shows a photograph of the very simple pit-like nest excavated by the male and female African mouthbreeder fish, *Tilapia macrocephala*. This pit serves as a target in which the female deposits the eggs. The male promptly fertilizes them and then picks them up to be carried in his mouth where the young will hatch. The nest is not used at any later time.

The nest of the ten-spined stickleback shown in Figure 3 is much more elaborate and serves a different function. This is constructed by the male using small bits of plants as building material and cementing them together with a substance secreted by the kidney. The female is induced by the male's courtship to swim inside the nest where she deposits her eggs and then leaves. Thereafter the male fertilizes the clutch, guards the nest, and assiduously fans the eggs with his fins.

There is no need to multiply examples; but similar comparisons in a variety of kinds of behavior could be made among invertebrates, amphibians, reptiles, and mammals. Within the primates each species of monkey and ape has its own characteristic behavior patterns which serve to identify it as a member of the primate order and at the same time to differentiate it from other species within the same group. What then can be said to typify man as far as species-specific behavior patterns are concerned?

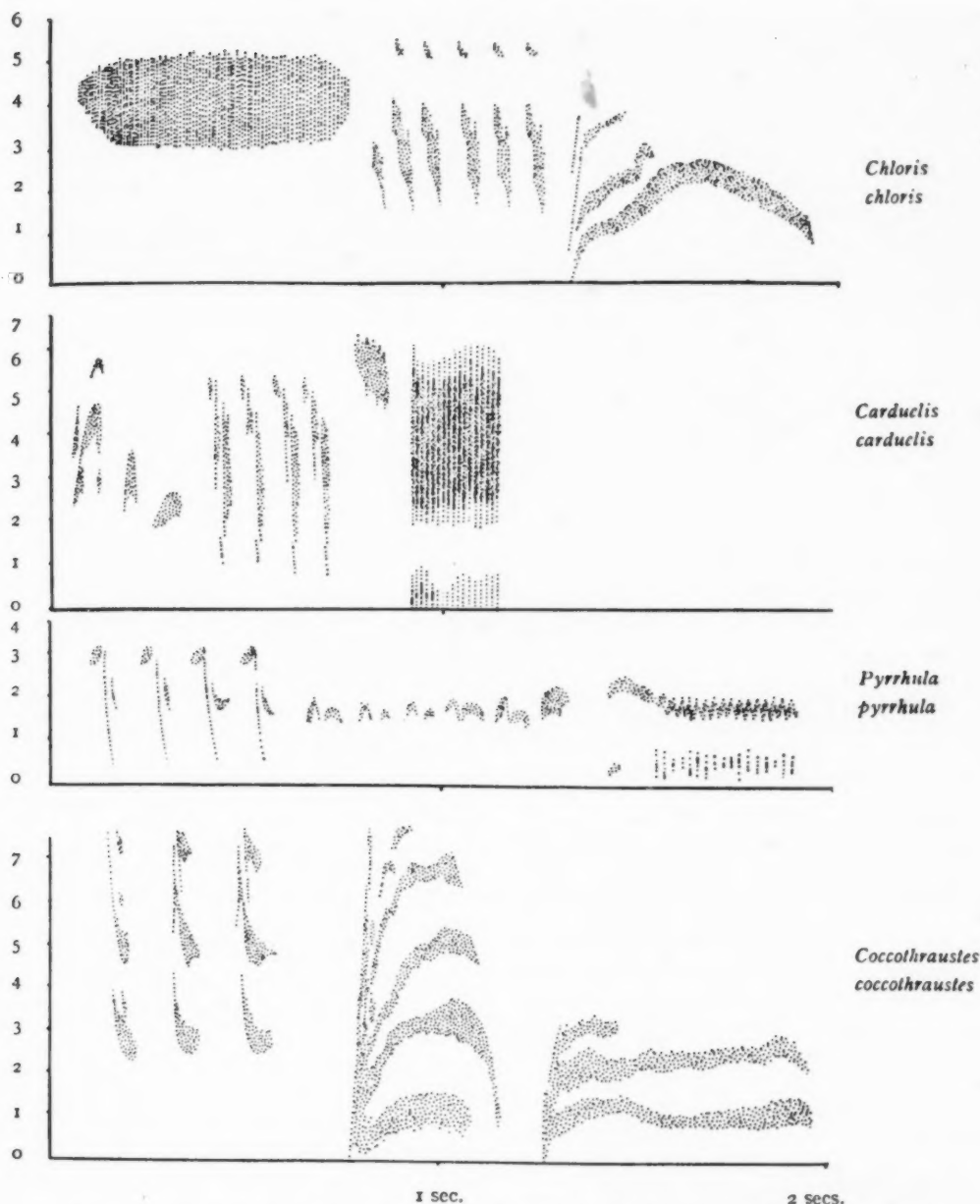


FIG. 1. Sound spectrograph tracings of the songs of four species of finches. (From Marler, 1957)

We have already seen that Simpson regards man as pre-eminent in intelligence, flexibility, individualization, and socialization; but these are not types of behavior comparable to nest building, mating, etc. Unquestionably the behavioral char-

acteristic which sets man apart from all other animals is the possession of language. Many other species *communicate* through vocalizations and gestures, but none possesses anything approaching human language in modifiability and complexity.

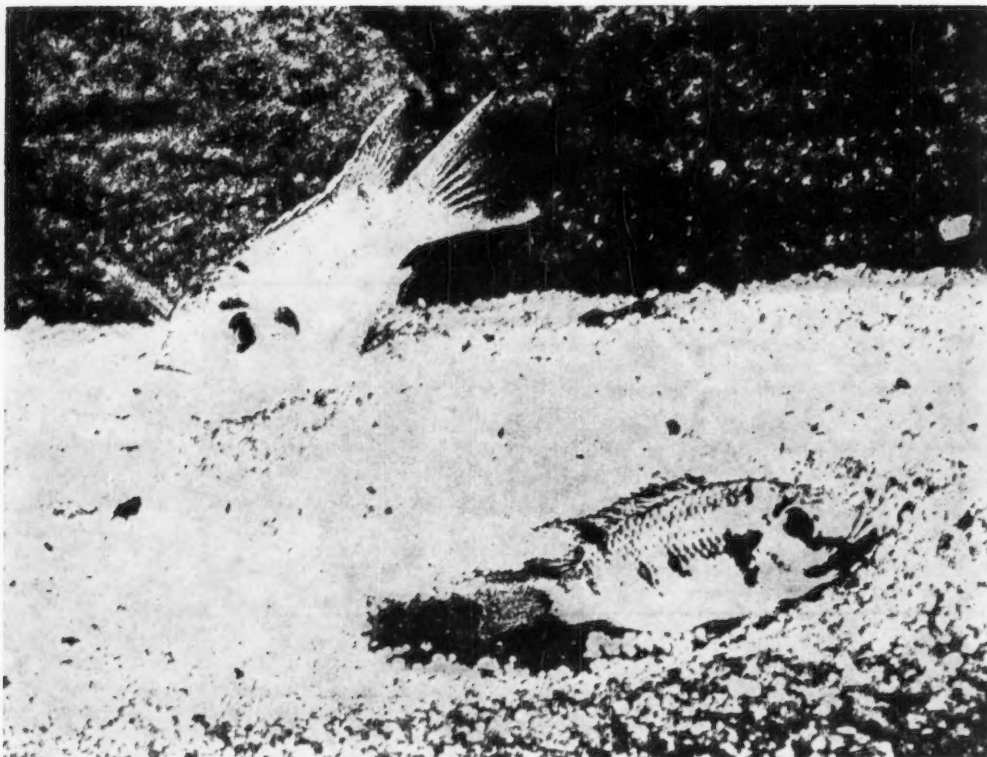


FIG. 2. An example of the simple, pit nest excavated by the African mouthbreeder fish. (From Aronson, 1949)

As a matter of fact, the single diagnostic character of language is sufficient in and of itself to identify an organism as a member of our species. Hebb and Thompson (1954) say that man is the only

animal capable of "syntactic" behavior. It is important to note that language serves as more than a means of communication. A great deal of human thinking and problem solving is done in terms of language symbols. By virtue of language man is also unique in his capacity to anticipate remotely future events, possible or inevitable. This is at times advantageous, but it can also produce crippling anxieties.

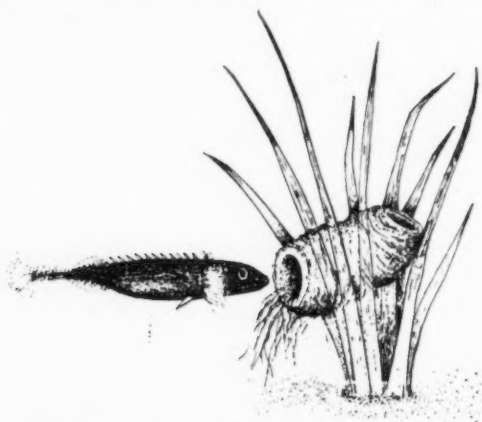


FIG. 3. The nest of the ten-spined stickleback. (From Morris, 1958)

Language depends upon and, at the same time, underlies another one of man's outstanding characteristics: namely, the capacity to establish a wide variety of exceedingly complex social interrelations. Language and the formation of social groups permit the transmission of information from one generation to the next and thus give rise to a new type of evolution not seen in any other species. Social evolution, based upon the intergeneration transfer of knowledge, customs, and beliefs, has resulted in the growth of culture—another uniquely human characteristic.

The conclusion seems inescapable that a comparative behaviorist who wished to concentrate upon the species-specific characteristics of mankind would emphasize those aspects of behavior depending upon the use of language and involving interpersonal relations and the formation of social groups.

PLAN OF ATTACK

It may be agreed that some kind of scientists, not necessarily psychologists, might find out some interesting, important—conceivably even useful—things by studying species-specific behavior in animals; but there may nevertheless be some doubt as to precisely what it is that these hypothetical investigators are going to do. How are they going to proceed once they have chosen a particular pattern of behavior for scientific study?

The general plan of attack is deceptively simple. One merely attempts to answer three questions: How did the behavior get to be what it is? What are the external causes of the behavior? What internal mechanisms mediate it? Phrased in slightly more sophisticated terms each of these questions can be said to deal with a different class of *determinants* of behavior. The determinants of behavior can be subdivided into three broad categories: historical determinants, environmental determinants, and organismic determinants.

In more familiar language, *historical* determinants are in turn subdivisible into two classes which are usually referred to as maturational and experiential factors. *Environmental* determinants may be either *direct* or *indirect*. Direct environmental determinants are easy enough to understand. Traditionally defined as stimuli, they are those external factors or correlates which are often said to evoke the behavior. Usually there is a fairly clear-cut and immediate relationship between presentation of a direct environmental determinant and the occurrence of the associated behavioral response.

Indirect environmental determinants will be somewhat less familiar to many psychologists, and an example will help to clarify the concept. Figure 2 illustrated the simple pit-like nest which is dug by the African mouthbreeder fish prior to spawning. The male does some nest digging, but the female performs most of the work. Females that are completely isolated do not dig nests. However, an unmated female, in a tank by herself, will dig

a nest if she has been able to see another mouthbreeder in an adjacent tank for several days. Not only that—she will then proceed to lay eggs in the nest.

In painstaking analysis, Aronson (1945) has identified the essential determinant as being visual and only visual. The sequence of events seems to be roughly as follows. Constant exposure to the sight of others of her own species causes the ovaries of the female to become active. Eggs ripen, and ovarian hormones are secreted. As the female's physiological condition changes, her reactions to certain aspects of her environment change. The sandy bottom of the tank now becomes something to dig in, and nest building begins. It appears that the actions involved in digging—a process which may stretch over many hours—have further effects upon the ovary, effects which eventually give rise to ovulation and oviposition.

In this example, the prolonged visual access to another *Tilapia*, prior to the onset of the behavioral response, is an indirect environmental determinant of nest digging behavior. It is a necessary but not sufficient factor. The *direct* environmental determinant of the response is sight of a sandy or light colored bottom. Females kept in aquaria with bare slate floors show no nesting activity; but, if the floor is covered by a layer of sand, and the sand in turn is covered by a sheet of clear glass, then the female exhibits persistent digging responses, even though she cannot touch the sand.

The third class of determinants is a very familiar one. Organismic determinants of behavior form the subject matter of physiological psychology. To define them crudely, they are the determinants that lie inside the individual. The most frequently investigated organismic determinants to date have been those of a neural or an endocrinological nature.

These, then, are the three classes of determinants of behavior: historical, environmental, and organismic. Now it will be apparent that these three classes are not completely separate and surely do not operate independently. The effectiveness of a direct environmental determinant will be a function of other determinants of an historical nature. The influence of an indirect environmental determinant is exerted by way of organismic changes. Organismic determinants, in turn, influence responses to environmental variables. It is nevertheless useful in the beginning to treat the three classes as if

they were clearly separable. They are the factors the comparative behaviorist seeks to identify in his study of species-specific patterns in different kinds of animals. He may discover that the same kind of behavior in different species depends upon the same or upon different combinations of determinants, and these similarities and differences will provide an essential basis for the understanding of interspecific differences and similarities at the molar level.

At this point I would like to make one very general and, I believe, very important point concerning the analysis of behavior. I have said that the comparative behaviorist accepts as an important goal the identification of the several types of determinants of species-specific behavior. The point I wish to emphasize is that *precisely the same kind of analysis is necessary for the understanding of any kind of behavior exhibited by any kind of organism*. The psychologist interested in the language development of children or in the onset and course of homesickness in college freshmen must, if his knowledge of the phenomena is to be complete, ask the same questions as are asked by the comparative behaviorist when he studies the migration of birds or the cooperative behavior of chimpanzees.

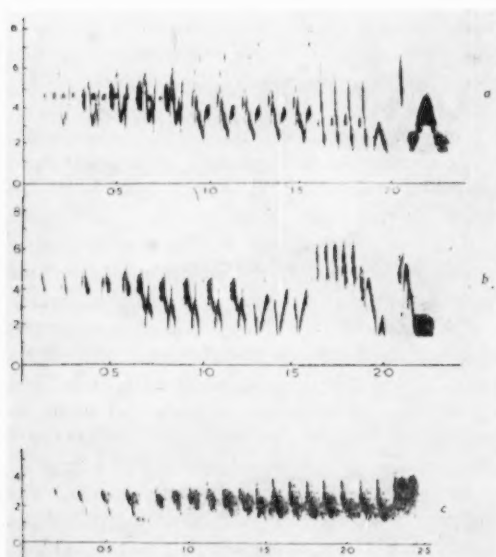


FIG. 4. Sound spectrograph recordings of the song of two wild-caught chaffinches (a and b) and a hand-reared bird of the same species (c). (From Thorpe, 1956)

DETERMINANTS OF BEHAVIOR

Up to this point I have dealt in rather general terms with the determinants of behavior. The following description of specific observations and experiments will illustrate methods of identifying these determinants. These examples have been selected to represent the greatest possible range of behavior patterns and to include the widest phylogenetic spread within the vertebrate phylum.

Historical Determinants

As an illustration of the analysis of historical determinants we find an excellent example in Thorpe's study (1956) of the development of song in the chaffinch. The song is highly species-specific. Young chaffinches remain in the nest until late summer; and during the fledgling period they constantly hear adults singing, although the young themselves are too immature to sing. During the following spring, when their reproductive glands start secreting sex hormones, the birds begin to sing; and they produce the normal, full, chaffinch song. Since immature birds do not sing at all, but reproductively mature ones *do* sing, maturational factors constitute some of the historical determinants involved.

Figure 4 illustrates a sound spectrograph recording in which frequency or pitch is shown on the ordinate—the higher the marks, the higher the tone. Time is shown from left to right. The total duration of the song is about 2.3 seconds. The song of two wild-caught chaffinches is shown in the two top records. Some experience is needed to recognize it, but the song consists of three phrases, the third phrase ending in a fairly complex terminal flourish. Thorpe wished to identify the factors governing the development of song in the individual, i.e., its historical determinants.

Taking young chaffinches from the nest a few days after hatching, Thorpe reared them individually in soundproof cages where they could never hear any bird songs. When they came into breeding condition the following spring, these birds began to sing; but the song produced was like that shown in the bottom record of Figure 4. It resembles the species-specific song in only two respects. It is of normal duration, and there is some tendency for the song to end on a higher note. There is no phrasing, and the frequency range is greatly restricted. After this song has been sung

INVESTIGATIONS OF SPECIES-SPECIFIC BEHAVIOR

at full intensity for a few days, it becomes fixed; and thereafter exposing the singer to normal chaffinches in full song has no effect.

Apparently the opportunity to hear other birds singing is a necessary historical determinant of the development of normal species-specific vocalization. It is possible that in nature young birds coming into their first breeding season hear experienced singers and acquire the species song as a result. That this is not a satisfactory explanation is shown by a second experiment.

Thorpe removed chaffinches from the nest at the end of the summer, just before they would have normally departed. It must be remembered that, throughout the fledgling period, these birds had been exposed to the singing of adults, but had never sung themselves. Groups of such birds were placed in aviaries where the only songs they could hear were those uttered by other members of their own experimental community. In contrast to birds reared in complete auditory isolation, these individuals developed a fairly elaborate song. Three features of their song were noteworthy: Phrases 1 and 2 were practically normal for the species, the ending of Phrase 3 was abnormal, the abnormal pattern of Phrase 3 was almost identical for all members of the same experimental community.

After the experimental birds had sung their full song for only a few days, the pattern became stabilized and remained relatively immutable. Figure 5 shows the song of a chaffinch reared under the conditions just described. The top record was taken in 1951; the bottom record was made in 1953. Between the time of the first and second recordings this bird lived in an outdoor aviary where he was constantly subjected to a barrage of normal chaffinch song produced by wild members of his species. The figure shows that this had absolutely no modifying effect upon his abnormal song pattern.

The similarity in Phrase 3 as sung by all members of the same experimental community suggests that the birds were learning from each other. But why are Phrases 1 and 2 typical of the species-specific song? Two possibilities present themselves. The first is that these parts of the song do not depend upon learning in the usual sense, but nevertheless for them to appear it is necessary that the individual be able to hear other birds singing—the exact pattern of the song being immaterial. This would explain why the chaffinches reared in

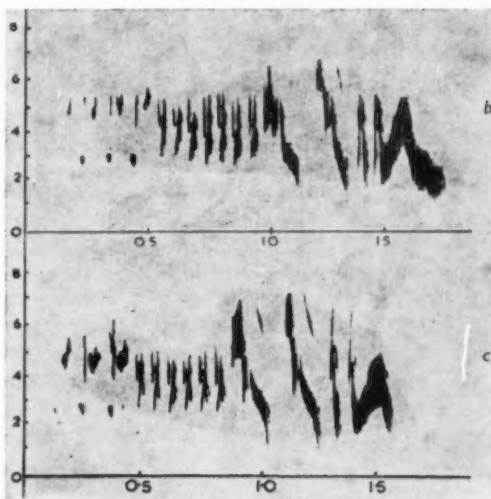


FIG. 5. Song of a chaffinch isolated as a juvenile and reared in isolation from adults. (From Thorpe, 1956)

complete isolation failed to develop Phrases 1 and 2. The second possibility is that the experience of hearing the species-specific song during the fledgling phase of life has an effect which shows up months later when the experimental birds first begin to sing.

Improbable as it may seem, the second interpretation, which is reminiscent of latent learning, appears to be correct. Thorpe took chaffinches from the nest very early in life and hand reared them in groups. They were thus unable to hear adults singing during the fledgling period. They could hear each other, but no other birds. The kinds of songs that developed are represented in Figure 6. The two top records are those of two chaffinches from one experimental group; the bottom two records show the songs of a pair of birds from another experimental community. None of these records contains the species-specific Phrase 1 and Phrase 2. The highly elaborate phrasing in the two bottom records is impressive, but these songs are exceedingly unlike anything ever recorded from wild birds.

These findings, taken in conjunction with the results of removing birds from the nest at the close of the fledgling period, support the working hypothesis that under normal conditions exposure to the species-specific song in infancy is responsible in part for the development of the normal song during the following spring. They also show quite clearly

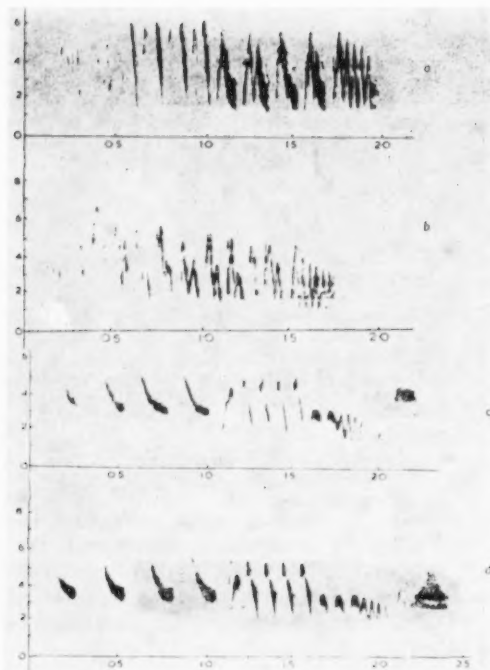


FIG. 6. Sound spectrograph recordings of four chaffinches removed from the nest at about the fourth day of life and then reared in experimental communities where they could hear only each other's songs. (Records a and b taken from one community, c and d from a different community—from Thorpe, 1956)

that under experimental conditions inexperienced singers can learn from each other.

I have spent some time presenting this example for two reasons. First of all, it illustrates the extreme complexity of the historical determinants of what is, after all, a fairly simple response. Secondly, I believe that it supports my contention that species-specific behavior in different species can be studied by the same methods with valuable results. For example, Thorpe's general methods could, theoretically, be applied to the study of language development in children. For obvious, but entirely nonscientific reasons, this is not feasible; but if it were, the results would undoubtedly be extremely illuminating and of great importance to the understanding of human behavior.

Quite comparable methods have been utilized in analyzing the phenomenon of imprinting which involves the tendency of the young of gregarious species to follow and remain near adults of their

own kind. Studies of very young birds and mammals which show this tendency have revealed that the *following* response is spontaneous and unlearned, but early experience determines what type of object will be followed. There appears to be a primitive sort of learning which normally takes place during a critical period of infancy. If learning is prevented during this period, it cannot take place at a later time. And if learning does occur, the results are exceedingly resistant to extinction or reversal.

Eckhard Hess (1959) has studied imprinting in mallard ducklings using the apparatus shown in Figure 7. The stimulus model moves around a circular course, and the experimental subject is free to follow. After having been exposed to the model for a predetermined length of time, the duckling is removed and retested after an interval for strength of imprinting. Strength of imprinting means simply strength of the tendency to follow the model at the time of the second exposure.

Hess has confirmed earlier reports to the effect that susceptibility to imprinting varies with the age of the duckling. If the initial exposure to the model is delayed until the little birds are several days old, no imprinting occurs. This effect is illustrated in Figure 8. In the graph on the left, the values on the abscissa represent the age at which the ducklings were initially exposed to the model—that is, the age at which they were imprinted. On the ordinate is shown the percentage of birds that followed the model when they were tested at a later time. It is apparent that the tendency to follow was strongest in ducklings that had been imprinted between 13 and 16 hours after

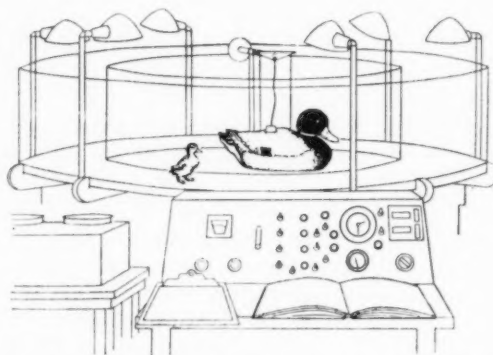


FIG. 7. Apparatus used to study imprinting in ducklings. (From Hess, 1959)

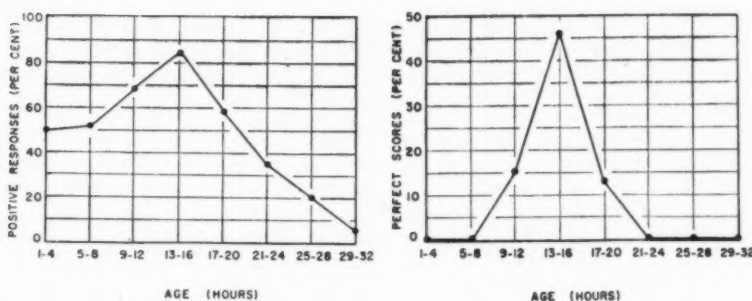


FIG. 8. Strength of imprinting as a function of age. (From Hess, 1959)

hatching. Very little following was shown by birds whose initial exposure to the model had been delayed for 29-32 hours after hatching.

It appears that there is a critical period very early in life when the birds are highly susceptible to imprinting. Once this period has passed, the possibility of obtaining the effect is greatly reduced. This is illustrated by the right-hand graph in Figure 8. Here again the age at which initial exposure to the model occurred is shown on the abscissa, and the strength of imprinting as measured at a later time is indicated on the ordinate. In this figure imprinting strength is expressed in terms of the percentage of birds that had perfect scores—that is, that followed the model closely and continuously at the time of the retest. On the basis of this more rigorous criterion only those individuals exposed to the model when they were between 9 and 20 hours old gave evidence of having been imprinted. Earlier and later exposure was ineffective. The period of greatest sensitivity was 13 to 16 hours after hatching.

Hess has gone further in his analysis of the historical determinants of the following response and

has shown that the strength of imprinting depends heavily upon the amount of energy expended in following the model during the period of initial exposure. Merely allowing a duckling to see the model will not produce imprinting. It is essential that the young bird exert itself and spend its efforts in remaining near the model.

The graph in the left half of Figure 9 illustrates this effect. During the period of initial exposure some birds were required to walk only 13 feet in order to stay close to the model. Others were forced to travel greater distances ranging up to 100 feet. The strength of imprinting as tested after a standard delay is indicated on the ordinate. It is obvious that strength of imprinting was positively related to distance traveled—hence energy expended—when first exposed to the stimulus model.

The right-hand graph supports this conclusion and shows further that the length of time spent with the model during imprinting is less important than the factor of work performed. Birds left with the model for 30 minutes, but required to walk only 12.5 feet during this period, showed less imprinting

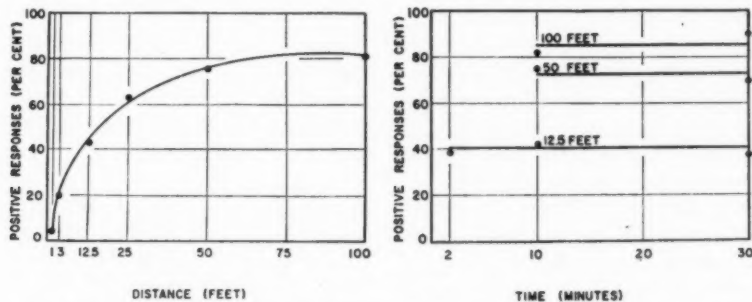


FIG. 9. Strength of imprinting as a function of distance traveled or time spent with the model. (From Hess, 1959)

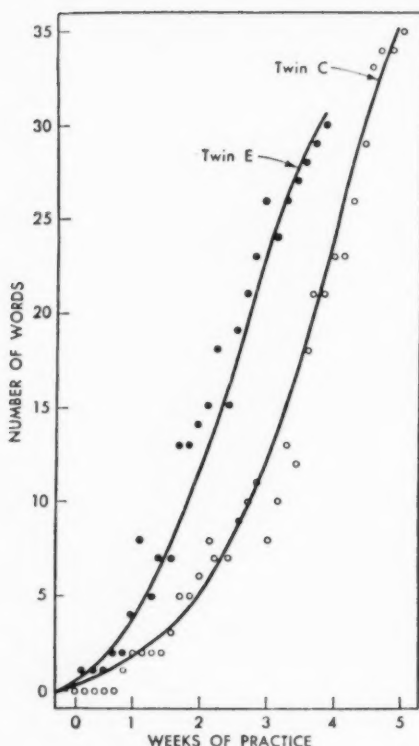


FIG. 10. Number of words in the vocabularies of twins as a function of weeks of practice. (Twin E began practicing five weeks later than Twin C—from Strayer, 1930)

than birds left with the model for 10 minutes, but required to walk 50 or 100 feet during the brief period of exposure.

The studies of Thorpe on singing in the chaffinch and the work of Hess and others on imprinting have certain findings in common. They suggest that the normal development of certain species-specific behavior patterns depends upon particular types of experience gained early in life. They suggest further that in the course of development the individual passes through one or more stages of maximum sensitivity to such types of stimulation or experience. Finally, they suggest that, if the critical experience is not gained during the period of high sensitivity, the normal species-specific behavior does not develop.

The comparative behaviorist would like to know whether any comparable phenomena are discovered in human ontogeny. I believe that they exist and that they operate in the development of man's

unique, species-specific characteristic, namely, the mastery and use of language.

It is clear that very young infants are incapable of understanding language; but by the second year of life words are uttered, the vocabulary grows, and words obviously acquire meaning. An extreme environmentalistic interpretation might be that the initial delay is due to inexperience and that language development depends entirely upon experience and practice. Strayer's study (1930) of growth of vocabulary in twins indicates that this is not the case. When the twins were 84 weeks old, the experiment was begun. Each day Twin C was given 75 minutes of language training. This consisted of attempting to induce the twin to name simple objects and to follow simple, verbal instructions. During the experimental period Twin E was also given training, but this training did not involve the use of language. After nine weeks of differential treatment Twin C showed a considerable degree of language development, being able to respond appropriately to approximately 35 words.

At this time Twin E was tested and demonstrated no improvement over her performance five weeks earlier. Now Twin E began to receive the same kind of training which had previously been given to Twin C. The results are shown in Figure 10. It is important to note that the values shown on the abscissa indicate weeks of practice, and this means that the values for Twins C and E signify scores made at different chronological ages. At the zero point on the graph Twin C was 84 weeks old, and Twin E was 89 weeks old. By the fourth week Twin C was 88, and Twin E 93, weeks old. The important point is that the curve for Twin E rises very rapidly, so that by the end of the experiment the two individuals were performing approximately equally. There is no score for Twin E on the fifth week because this child became ill at that point. The graph shows that, when Twin E received language training, the development was faster than that of Twin C who had been given practice earlier in life. Kimball has summarized these results as follows:

This result is typical of experiments of this kind: restriction from normal practice will produce a temporary deficit in the skill to which this restriction applies; when practice begins, the deficit disappears, and the experimental twin rapidly catches up with the control. One incidental finding of this particular study is that language development, more than might be expected, depends upon maturation (Kimball, 1956, p. 188).

The mastery of written language comes later than the acquisition of speech. There is no doubt that practice is very important, but the ability to improve as a result of practice is heavily dependent upon maturation. Figure 11 shows the findings of Morphett and Washburn (1931) with respect to the influence of mental age upon reading ability. The crucial point illustrated here is the very rapid increase between 5.5 and 6 years. Evidently intellectual development must proceed to a crucial minimal level before learning can begin.

The data discussed thus far show that certain levels of maturation must be reached before the species-specific character, namely, language behavior, can begin to develop as a result of experience. The next query is whether there is an upper limit after which language acquisition becomes impossible, or at least exceedingly difficult. In other words, is there a critical period for language development. We can only speculate on this question; but there is some exceedingly fragmentary evidence suggesting that, if language acquisition is prevented during the years it would ordinarily occur, later exposure to language does not result in normal language development.

Reports describing so-called feral children must be regarded with great skepticism, but one universally observed defect in such cases is the inability to learn language. Somewhat more reliable evidence has been published by Spitz (1945, 1946). He found that human infants reared under conditions of extreme social deprivation were very

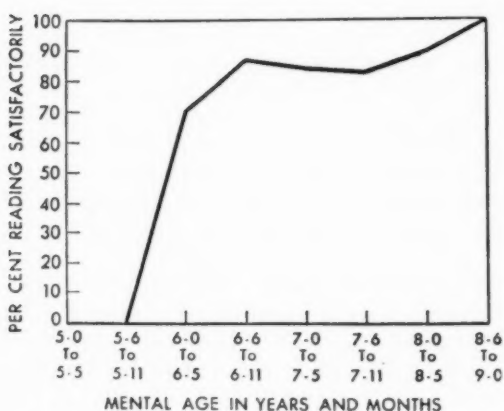


FIG. 11. Percentage of children reading satisfactorily as a function of mental age. (From Kimball, 1956, based on data published by Morphett and Washburn, 1931)

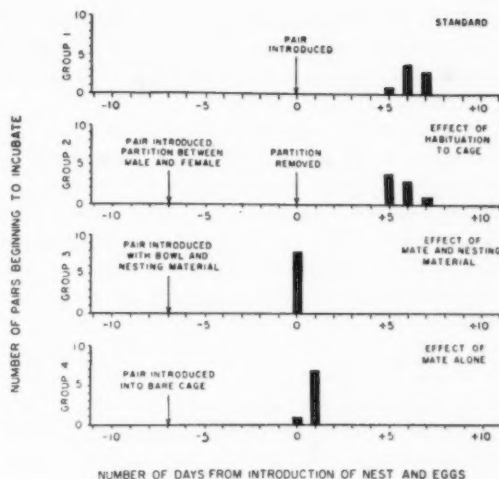


FIG. 12. Stimulus conditions contributing to the initiation of incubation in the ring dove. (From Lehrman, 1958)

slow in learning to use and understand words. If the period of deprivation lasted for several years, the ability to build up a vocabulary and to form sentences was, apparently, permanently impaired.

There are many other examples of historical determinants of a variety of behavior patterns, but limitations of space dictate that we move on to a consideration of the identification of the environmental determinants of species-specific behavior. I shall first discuss indirect environmental determinants.

Indirect Environmental Determinants

In defining this class of determinants I referred earlier to Aronson's studies on the control of nest digging and oviposition in the female mouthbreeder fish. At this point I wish to describe an experiment by Lehrman (1958) using the ring dove, *Streptopelia risoria*, as a subject.

The reproductive pattern of the ring dove consists of a sequence of overlapping stages. These are, in order, courting, copulating, nest building, egg laying, incubating the eggs, and, finally, feeding the young after they hatch. Lehrman's objective was to identify the environmental factors that determine the tendency to incubate eggs. He first showed that male or female doves placed singly in experimental cages containing a nest bowl and eggs never showed any incubation. In other words, a mate must be present. Lehrman was therefore led

to ask: "Does participation in courtship and in nest-building activity help to bring about the readiness to incubate?" Lehrman's methods and results are shown in Figure 12.

The top graph represents the standard condition. Successive days are indicated on the horizontal axis. A male and female were put into the experimental cage at Day 0. The cage contained a nest bowl holding a nest and eggs. Incubation began from five to seven days later. Why did the birds not begin to incubate immediately? Several explanations for the delay are possible. Perhaps it took them that long to become accustomed to the cage. The second graph disproves this hypothesis. The pair remained in the cage for seven days, but were separated from each other by an opaque partition. On Day 0 the partition was removed, and a nest bowl containing a nest and eggs was introduced. Incubation did not occur until five to seven days later.

The third graph shows the effect of intrapair interaction plus nest building. In this experiment the male and female spent seven days in the experimental cage with nesting material and an empty nest bowl, but no eggs. The pair could thus engage in courtship, copulation, and nest building. They did all three of these things. On Day 0 the original nest bowl and the nest that had been built in it were removed, and a new bowl containing a nest and eggs was introduced. Incubation began within a few minutes in every case. This raised the question as to whether the immediate readiness to incubate was due to the preceding sexual activity, or to the preceding nest-building activity, or to both.

The final graph gives us the answer. Pairs were allowed to spend seven days in the experimental cage where they could court and mate, but not build a nest, because no nesting material was available. When a nest bowl with nest and eggs was introduced on Day 0, these doves did not immediately begin to incubate as had those in the preceding experiment. Instead they began actively to engage in nest building behavior, despite the fact that a ready-built nest was already present. After eight hours of feverish nest building one pair began to incubate, but the remaining seven pairs continued to nest-build and did not incubate until the following day.

It seems that several days of courtship and copulation are essential to the occurrence of incubation

and that a shorter period of active nest building must also occur. Sexual activity and nest building are, therefore, indirect environmental determinants of the incubation response.

The effects of indirect environmental determinants are quite as important in mammalian behavior as they are in the case of fishes and birds. The lactating female's maternal tendencies are strongly influenced, in indirect fashion, by the nursing activity of the young. At the human level many psychosomatic complaints and behavioral symptoms depend upon exceedingly indirect environmental determinants.

Direct Environmental Determinants

Having considered the case of historical determinants and of indirect environmental determinants, I am now prepared to present a few examples of studies aimed at identification of the direct environmental determinants of species-specific behavior.

The European and British ethologists have been extraordinarily successful in identifying direct environmental determinants by the use of models, or dummy stimulus objects. A simple illustration is provided by Tinbergen's study of aggression in the stickleback. During the mating season males of this species viciously attack other males that are in breeding condition. The male that is ready to reproduce is distinguishable by virtue of the bright red coloration of his ventral body surfaces.

Tinbergen presented different models to breeding males. These dummies are shown in Figure 13. Model N was an accurate copy of a male stickleback, but it lacked the red belly. This dummy elicited very few attacks. In contrast, all of the other models, which varied widely in shape, but shared the common characteristic of a red underside, evoked constant and vigorous attack by the experimental males. Apparently the crucial direct determinant in this case is the red belly.

Hebb has discussed the sudden appearance of fear responses in young chimpanzees and human children. In some cases the direct environmental determinants have been identified and found to be exceedingly simple. One case will illustrate the point. An eleven-month-old baby girl suddenly began to exhibit extreme fright reactions in a variety of situations which at first seemed to have nothing in common. When an old friend entered

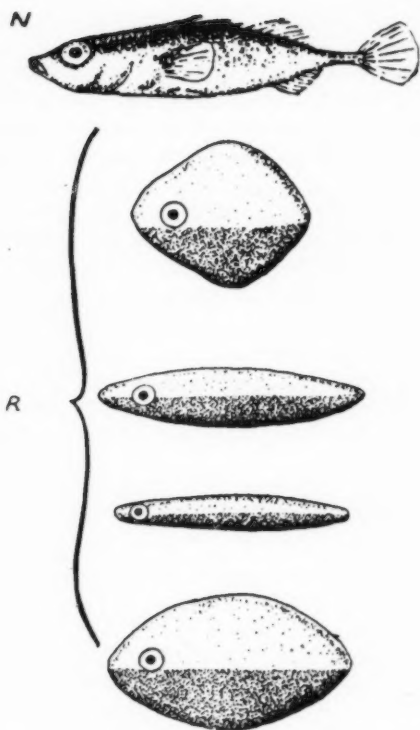


FIG. 13. Artificial models used to elicit aggressive behavior in the male stickleback. (From Tinbergen, 1951)

her playroom wearing a new hat, the child burst into tears and would not allow herself to be picked up until the hat was removed. Upon being presented with a new teddy bear she showed violent avoidance reactions and screamed with fright.

Examination of the child's reaction to a variety of stimulus objects revealed that the key factor evoking fear was color. Anything that was black elicited avoidance and great distress. Black hats, toys, or other objects were equally effective. Similar stimuli that were not black were accepted and examined with interest. The following figures illustrate the phenomenon. Figure 14 shows alert attention to the approaching experimenter. Figure 15 illustrates the reaction to a black glove placed gently on the tray of the high chair. This peculiar fear behavior disappeared as suddenly as it had appeared.

Psychologists have made relatively few attempts to identify the direct environmental determinants of species-specific behavior in human beings. In

his book, *Expression of the Emotions in Man and Animals*, Charles Darwin (1872) listed *smiling* under "Special Expressions of Man," and smiling probably is a species-specific pattern. Older children and adults smile in a variety of circumstances; but, in very young infants, the smile is a reflexive response to a relatively simple stimulus, a fact well-known to anyone who has played peek-a-boo with a baby.

Apparently the direct environmental determinant is visual and consists of a human face presented full view. Spitz and Wolf (1946) exposed 145 infants between the ages of two and six months to a variety of stimulus patterns in an attempt to evoke smiling. The only effective stimulus for babies of this age was a human face viewed from the front. A profile view did not elicit smiling. The stimulus face does not have to be smiling. In fact, the expression on the face proved to be irrelevant. Thus the stimulus shown in Figure 16 was quite effective as a smile producer. So was a face covered with a Halloween mask. Even a dummy fitted with a face-like mask regularly elicited smiling. When they are several months older, children begin to smile in response to other



FIG. 14. Expression of the eleven-month-old subject prior to introduction of the fear inducing stimulus.



FIG. 15. Reaction to a black glove.

stimuli; but under the age of six months the only direct environmental determinant appears to be full view of the human face or a reasonable facsimile thereof.

This simple, reflexive response in human babies is like a wide variety of more complex behavioral reactions shown by fishes and birds, in that they are evoked by relatively uncomplicated, direct environmental determinants, often involving only one sensory modality. In apparent contrast, complex species-specific patterns in mammals seem to depend upon patterns or combinations of direct determinants. Studies of retrieving of their young by lactating rats will serve as an example (Beach & Jaynes, 1956).

When she delivers her first litter, the inexperienced female rat normally cleans her young and gathers them into the nest that she built prior to parturition. If the experimenter scatters the litter about the cage, the mother promptly and efficiently retrieves them to the nest. I was interested in discovering the sensory cues utilized by the mother in locating and identifying her young. The first step was to examine the possible role of each sensory avenue.

The possible importance of odor was tested in three ways. First, it was shown that females could respond to the odor of the young by demonstrating that a blinded female could locate pups which were suspended high above her by a fine thread.

In order to reach these pups the female had to stand up on her hind feet. Secondly, blind females were allowed to investigate two wire mesh boxes, one empty and one containing a pup. The clear-cut preference for the latter box indicated a response to odor. Thirdly, I tested the response of normal females to pups which had been sprinkled with perfume. These pups were retrieved, but not until after the mother had found and retrieved the control pups which did not smell of Chanel No. 5.

Obviously, olfactory cues are an important environmental determinant of the retrieving response, but are they essential? The answer is an unequivocal "no." I destroyed the olfactory bulbs in a number of females, thus rendering them anosmic, and they retrieved just as efficiently as normal mothers.

Turning next to the investigation of visual cues, it was first established that lactating rats can locate their young exclusively on the basis of sight. The test involved presenting the mother with two small glass bottles, one containing a pup and one empty. The bottles were sealed airtight. Most females spent more time investigating the bottle containing a pup than the empty bottle. This suggested that the visual cues were important. Next the retrieving behavior of blind mothers was examined. They



FIG. 16. Visual stimulus effective in evoking the smiling response in young infants. (From Spitz & Wolf, 1946)

were slightly slower than sighted females, but the quality of their performance was excellent. It would appear that visual cues are used when they are available, but sight is not essential to retrieving.

Other senses were investigated, but the results on smell and vision are sufficient for the point I want to make. A given type of sensory cue may contribute to the occurrence of a response, and yet not be essential as long as alternative cues remain available. If two cues are eliminated, this is much more likely to interfere with the response. Thus, when I rendered the same females both blind and anosmic, their retrieving became much less efficient; and the added elimination of the sense of touch in the snout region almost abolished retrieving behavior.

Quite comparable results were obtained in my laboratory in studies of the sensory basis for the male rat's sexual responses to the receptive female (Beach, 1942). Much more evidence is needed, but the data presently available suggest that mammals may differ from other vertebrate classes in that their species-specific behavior depends upon direct environmental determinants which, instead of being very simple and involving only one modality, are multisensory in nature and may include some interaction between modalities. This, incidentally, is the sort of generalization which can be reached only by a broadly comparative approach to behavior.

Organismic Determinants

We have now examined, albeit briefly and incompletely, a few examples of the ways in which the comparative behaviorist studies the historical and environmental determinants of behavior. This leaves for final consideration the study of organismic determinants. This category could, logically, embrace every organ and organ system in the body; but in actual fact experiments have been concentrated almost exclusively on the nervous system and the endocrine system.

Neural determinants. To illustrate studies of the neural determinants of species-specific behavior I have chosen to summarize very briefly several experiments upon courtship and mating.

In work on mating in the male leopard frog, Aronson and Noble (1945) found that discrete regions of the brain and spinal cord control different elements in the total pattern. Figure 17

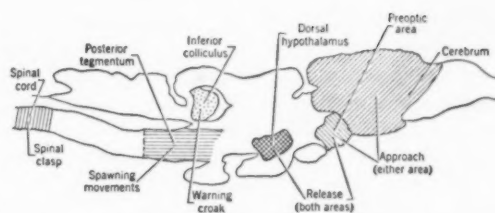


FIG. 17. Diagrammatic sagittal section through the brain of the leopard frog, showing some of the regions that mediate different elements in the male's mating pattern. (From Aronson & Noble, 1945)

shows diagrammatically a sagittal section of the frog brain. By removing different brain regions and then observing sexual behavior, Aronson showed that the male's approach to a female depends upon the intactness of the cerebrum or of the preoptic area. Clasping the female with the forelegs is mediated by spinal mechanisms. Spawning movements by the male while he clasps the female depend upon the posterior tegmentum, whereas release of the female after spawning is mediated by the preoptic area plus the dorsal hypothalamus. In higher vertebrates the control of individual elements in the total mating pattern is less discretely localized.

Studies of copulatory behavior in the male rat have indicated that the cerebral cortex is involved in this activity, although it apparently does not mediate any specific elements in the mating pattern (Beach, 1940). Following injury to the neocortex the probability that mating will occur is reduced in proportion to the total amount of cortical tissue removed regardless of the areas involved. Copulatory behavior is eliminated by very large lesions and unaffected by very small ones.

One very important finding was that males which continued to mate after brain operation did so in normal fashion. The chief effect of cortical injury was a reduction in excitability. Of considerable significance was the finding that some males which ceased responding to the female after a brain operation could be induced to resume mating if they were treated with very large amounts of androgenic hormone.

These findings have been taken to indicate that the principal effect of cortical lesions upon sexual activity in the male rat is to lower the responsiveness to sexual stimuli and thus to reduce the probability that sexual arousal will occur. Within cer-

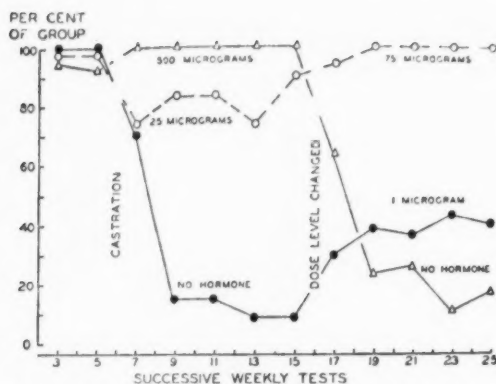


FIG. 18. Mating performance of male rats before castration and after castration while receiving different amounts of testosterone propionate. (From Beach & Holz-Tucker, 1949)

tain limits hypernormal concentrations of androgen can exert a compensatory effect.

In other investigations we have found that the effects of removing various parts of the cerebral cortex on mating behavior in the male cat are somewhat different (Beach, Zitrin, & Jaynes, 1955, 1956). This is not surprising since a variety of sensory-motor functions are more highly encephalized in the carnivore than in the rodent. In the cat, both the size and the locus of the lesion are important. Removal of the visual cortex prevents the animal from responding to the distant female on the basis of visual cues. Extensive injury to the motor and premotor areas renders exceedingly difficult the execution of the sensory-motor adjustments necessary for successful coition; and this operation practically prevents completion of the copulatory pattern without noticeably decreasing the male's interest in the estrous female. Total decortication renders the male cat unresponsive to the receptive female.

These experiments will have to suffice as illustrations of the study of neural determinants; but if space permitted, I could add many, many more. There have, for instance, been investigations of the neural control of courtship and mating in fish, birds, guinea pigs, dogs, and monkeys. Both the masculine and the feminine patterns have been studied, with the resultant disclosure of some unexpected and striking differences between the sexes. The kinds of behavior examined have included maternal care, food hoarding, fighting, eating, social inter-

action involving dominance or submission, sleeping, general activity, and exploratory behavior. Unfortunately I must be content merely to mention all of this important work and to move on to discuss the hormonal determinants of behavior.

Hormonal determinants. Here again one could spend several pages merely enumerating the studies dealing with this subject. Eleven years ago I attempted to summarize the pertinent literature, and the printed bibliography occupied 64 pages of the book. Since then the total has multiplied several times. Much experimentation has centered upon the hormonal determinants of the sexual activities of fishes, amphibians, reptiles, and birds. Within the class *Mammalia* work has been done on mice, hamsters, guinea pigs, rats, cats, dogs, cattle, monkeys, and apes. In addition to courtship and mating, other behavior patterns have been examined and their hormonal determinants at least partially identified. These patterns include parental care, hoarding, fighting, migration, general activity, and the establishment of social status.

To illustrate one general approach to the study of hormonal determinants it will suffice to describe, in capsule form, a few experiments on the effects of castration on male sexual behavior.

In the adult male rodent, removal of the testes is followed by a highly predictable decrease in sexual activity—culminating in the eventual loss of all but the most rudimentary and incomplete mating responses. The loss is permanent unless appropriate replacement treatment is given (Beach & Holz-Tucker, 1949). Figure 18 shows the effects of administering male hormone to castrated male rats. The values on the ordinate represent percentage of the group showing sexual behavior. Successive weeks are shown on the abscissa.

As shown by the first two points at the left end of each curve, all members of each group copulated before castration. Following the operation the controls (marked "no hormone") lost the ability to mate. The percentage of copulators decreased somewhat in the group of castrates that received 25 micrograms of androgen per day. Administration of 500 micrograms daily maintained performance at preoperative levels. As can be seen from the figure, when the original dosage levels were changed, sexual activity also changed, and in the expected direction.

In male dogs castration has a variable effect, depending upon the amount of sexual experience

acquired prior to operation. Experiments done in the laboratory at Yale showed that dogs which have had a great deal of successful copulatory experience in the testing situation may react to castration with no loss in excitability or potency for periods of as long as two years. In contrast, males with a minimum of sexual experience prior to gonadectomy tend to lose their ability to copulate within a few months after castration. They can, however, be restored to preoperative levels of potency if they are treated with androgen. Comparable results have been obtained with male cats by Rosenblatt and Aronson (1958).

The scanty evidence that is available indicates that male primates of infrahuman species are capable of a great deal of sexual activity in the absence of any testicular hormone (Beach, 1947). This is certainly the case as far as the human male is concerned; and comparisons involving rodents, carnivores, lower primates, and man give rise to the impression of a progressive decrease in the degree to which sexual performance depends upon testicular hormones.

CONCLUSIONS

Let me now summarize briefly the major points that I have raised. A core argument is that more research should be oriented to the naturally occurring behavior characteristic of various species. I have further suggested that this sort of orientation is probably essential to the development of a genuinely comparative science of behavior. This is regarded as a distinct change in emphasis from the current methods of animal research which concentrates on problems that have important implications for the understanding of human behavior, although they may have little relevance to the natural behavioral repertoire of the species that happens to be used. It is my thought that the proposed broadening of our orientation would facilitate and encourage rich interspecific comparisons and thus give rise to a truly comparative body of knowledge pertaining to behavior.

My hope is not for a decline in the use of animals in *psychological* research; rather, I am ambitious for the expansion of current programs to include what I have here recommended and, of equal importance, for the acceptance, on the part of those psychologists who use lower animals as substitutes for people, of the responsibility for demonstrating

the generalizability of their findings to *Homo sapiens*.

I close by voicing the anticipation that, if we remove man from the central point in a comparative science of behavior, this may, in the long run, prove to be the very best way of reaching a better understanding of his place in nature and of the behavioral characteristics which he shares with other animals as well as those which he possesses alone or which are in him developed to a unique degree.

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THE COGNITIVE ACTIVITY OF THE CLINICIAN

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SOMEbody has described psychotherapy as "the art of applying a science which does not yet exist." Those of us who try to help people with their troubles by means of that special kind of conversation are uncomfortably aware of the serious truth behind this facetious remark. The clinical psychologist has been able to assuage some of his therapeutic anxiety, and to refurbish his sometimes battered self-image, by keeping one foot planted on what seemed like comparatively solid ground, namely, psychodiagnosis. In recent years, some clinicians have been making a determined effort to assess the validity of our currently fashionable diagnostic instruments, and the findings are not very impressive. The cumulative impact of validation studies is reflected, for example, in Garfield's excellent textbook (1957), where one does not need a highly sensitive third eye to discern a note of caution (or even pessimism?). E. L. Kelly finds that 40% of young clinicians state that they would not go into clinical psychology if they had it to do over again. One suspects that at least part of this professional disillusionment springs either from awareness of the weaknesses in our psychodiagnostic methods or from the chronic intrapsychic (and interprofessional!) strain exacted of those who ward off such a confrontation. Who, for example, would *not* react with discouragement upon reading the recent monograph by Little and Shneidman (1959) where, in an unbiased and well-designed study, we find a very low congruency among interpretation of psychological test data, the test interpreters having been chosen as "experts" on four widely used instruments? Any tendency I felt to rejoice at the slight superiority of the MMPI over the three projective techniques with which it was competing was counteracted by the finding that my favorite test, like the others, does not do at all well when judged in absolute terms.

The cognitive activity of the clinician can be separated into several functions, which I have discussed in a recent paper (Meehl, 1959a). Setting aside for the moment that special kind of cognitive activity which goes on within the therapeutic inter-

view, we can distinguish three classes of functions performed by the psychodiagnostician: *formal diagnosis* (the attachment of a nosological label); *prognosis* (including "spontaneous" recoverability, therapy-stayability, recidivism, response to therapy, indications for differential treatment); and *personality assessment* other than diagnosis or prognosis. This last may be divided, somewhat arbitrarily, into *phenotypic* and *genotypic*: the former being the descriptive or surface features of the patient's behavior, including his social impact; the latter covering personality structure and dynamics, and basic parameters of a constitutional sort.

Quite apart from the validity of current techniques for performing these various cognitive functions, their pragmatic value is open to question. It is commonly believed that an accurate pretreatment personality assessment of his patient is of great value to the psychotherapist. It is not known to what extent, if at all, this is true. However, what do psychotherapists themselves have to say about it? Bernard C. Glueck, Jr. and I have recently collected responses from 168 psychotherapists (both medical and nonmedical, and representing a wide spectrum of orientations: e.g., Freudian, neo-Freudian, Radovian, Sullivanian, Rogerian, eclectic, "mixed") to a questionnaire dealing with 132 aspects of therapeutic technique. One of our items reads: "It greatly speeds therapy if the therapist has prior knowledge of the client's dynamics and content from such devices as the Rorschach and TAT." While the self-styled groups differ significantly in their response to this item (ranging from a unanimous negative among Rogerians to a two-thirds affirmative among George Kelly disciples), all groups except the last tend to respond negatively. The overall percentage who believe that such prior knowledge of the client's personality greatly speeds therapy is only 17%. This low figure, taken together with the fashionable de-emphasis upon nosology and the feebleness of most prognostic studies, at least raises doubts about the practical value of our diagnostic contribution.

Although they do not bear directly upon this

question, we have some other interesting results which suggest considerable skepticism among therapists as to the significance of causal understanding itself in the treatment process. For example, 43% state that "Warmth and real sympathy are much more important than an accurate causal understanding of the client's difficulty." Over one-third believe that "Literary, dramatic, aesthetic, or mystical people are likely to be better therapists than people of a primarily scientific, logical, or mathematical bent." Four out of five believe that "The personality of the therapist is more important than the theory of personality he holds." About half believe that "Interpretation as a tool is greatly overrated at present." Two out of five go as far as to say that "Under proper conditions, an incorrect interpretation, not even near to the actual facts, can have a real and long-lasting therapeutic effect." Time does not permit me to read other examples of items which, in the aggregate, suggest minimization of the importance of the therapist's forming a "correct" picture of the client's psyche.

Setting aside the pragmatic question of the therapeutic value of assessment, let us look briefly at the inductive structure of the assessment process. The epistemological rock bottom is a single, concrete, dated slice or interval in the behavior flux, an "episode," identified by certain physical or social properties. Having observed one or more episodes of a given kind, we make an inductive inference as to the strength of low order *dispositions* which these episodes exemplify. Such dispositions are grouped into families, the justification for this grouping being, as Cattell (1946, 1950) has emphasized, some kind of covariation (although not necessarily of Type R) among the members of the disposition-family. It is perhaps possible to formulate the clinician's decision making behavior entirely in terms of such disposition-classes. In such a formulation, clinical inference involves probabilistic transition from episodes to dispositions, followed by the attribution of further dispositions as yet unobserved. Ideally, such inferences would be based upon an extensive actuarial experience providing objective probability statements. Given a particular configuration of dispositions present in a patient, the statistical frequencies for all other dispositions of practical import would be known within the limits of observational and sampling errors. In practice, of course, this ideal is rarely achieved, the conditional probabilities being sub-

jectively judged from clinical experience without the benefit of an actual tallying and accumulation of observations, and the probabilities being expressed in rough verbal form, such as "frequently" and "likely," rather than as numerical values.

I am still of the opinion (McArthur, Meehl, & Tiedeman, 1956; Meehl, 1954, 1956, 1957) that the practical utility of this approach has been insufficiently explored, and I think that many clinicians are unaware of the extent to which their daily decision making behavior departs from such a model not by being qualitatively different but mainly by being less explicit and, therefore, less exact. However, we must recognize that a purely dispositional approach is not the *only* way of proceeding. An alternative, more exciting (and more congenial to the clinician's self-concept) is to view the clinician's cognitive activity as aiming at the assessment of hypothetical inner states, structures, or events which cannot be reduced to dispositions but which belong to the domain of theoretical entities, crude though the theory may be. Episodes and dispositions are here treated as "signs" or "indicators" of the postulated internal states. These states should not be spoken of as "operationally defined" in terms of the dispositions, because the logical relationship between propositions concerning theoretical entities and those describing dispositions is not one of equivalence, but merely one of degrees of confirmation. The inference from dispositions to states of theoretical variables is again only probabilistic, partly because statistical concepts occur within the causal model itself (i.e., probability appears, as in the other sciences, in the object-language) and partly because the theoretical network is incomplete and imperfectly confirmed.

A fundamental contribution to the methodology of inference from multiple indicators is the "multi-trait-multimethod matrix" of Campbell and Fiske (1959). These authors show that in order to support a claim of construct validity, we must take into account more kinds of correlational data than have been traditionally provided and that it is just as important for some correlations to be low as it is for others to be high. Consider two or more traits (e.g., dominance and sociability), each of which is allegedly measured by two or more methods (e.g., MMPI scores and peer group ratings). Computing all possible intercorrelations, we construct a multitrait-multimethod matrix. The rela-

tionships within this matrix may or may not lend support to the claim of construct validity. The monotrait-heteromethod coefficients should be not only statistically significant and respectable in size, but should exceed both the heterotrait-heteromethod and heterotrait-monomethod coefficients. For example, if MMPI dominance and sociability correlate higher than does MMPI dominance with peer group dominance or than MMPI sociability with peer group sociability, we ought to be nervous about the relative contribution of methods factors versus traits under study. Campbell and Fiske point out that the individual differences literature is very weak in this respect, usually failing to provide the necessary data and, when it does, usually showing unimpressive results.

An interesting adaptation of the Campbell-Fiske technique arises if we substitute "persons" for "traits" and deal with Q correlations rather than R correlations. Suppose that a therapist provides us with Q sort descriptions of two patients. From the MMPI profiles these patients are then Q sorted independently by two interpreters. This set up generates a modified Campbell-Fiske matrix of 15 Q correlations, in which the validity diagonals (i.e., heteromethod-monopatients coefficients) represent how similarly the same patient is perceived by the therapist and the two MMPI readers; the monomethod-heteropatients and heteromethod-heteropatients values reflect the projections, stereotypes, and other idiosyncratic sorting biases of the therapist and of the two interpreters, the extent to which such stereotypes are shared by all three, and the unknown true resemblance of the particular patient pair. Robert Wirt and I have been running a series of such matrices, and thus far our results are as unencouraging as those of the Little and Shneidman study. I have decided to spare you the slides, faintly hoping that the pairs thus far completed will turn out to be atypically bad.

The situation is not much improved by selecting a small subset of "high confidence" items before Q correlating. One disadvantage of Q sort is that it requires the clinician to record a judgment about every trait in the deck. The technique has the advantage that it presents the judge with a standard set of dispositions and constructs and therefore gets judgments which he is able to make but would often fail to make in producing a spontaneous description. But, for this advantage in coverage we

have to pay a price. Such a situation is clinically unrealistic: whether we are starting with test data, history, or interview impressions, the particular facets which stand out (whether high or low) will not be the same for different patients. It may be that the meager results of recent validation studies are attributable in part to the calculation of hit frequencies or Q correlations over the entire range of traits, only a minority of which, variable in composition, would willingly be judged by the clinician on any one patient.

I cited earlier the statistic that only one psychotherapist in six believes that he is greatly helped in the treatment process by having advance knowledge of the patient's psychodynamics. One relevant consideration here is the rate at which the psychotherapist's image of his patient converges to a stable picture. John Drevdahl, Shirley Mink, Sherman Nelson, Murray Stopol, and I have been looking into this question. So far, it seems that the therapist's image of his patient crystallizes quite rapidly, so that somewhere between the second and fourth therapeutic hour it has stabilized approximately to the degree permitted by the terminal sort-resort reliabilities. Let me show you a couple of typical results. Figure 1 shows the Q correlations between Stopol's phenotypic sort after the twenty-fourth hour and his successive sorts after the first, second, fourth, eighth, and sixteenth hours. " S_1 " indicates correlation of his stereotype with twenty-fourth-hour sort. "Rel" is sort-resort reliability. (The phenotypic and genotypic ratings are made separately.) Figure 2 shows results for the genotypic pool. I do not mean to suggest that the therapist's perception at the end

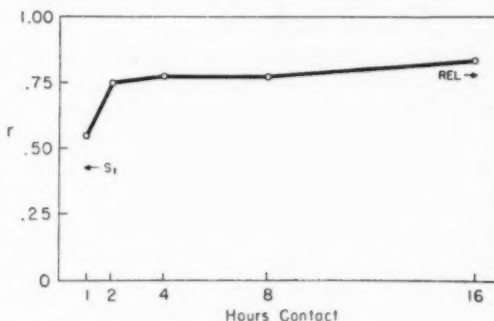


FIG. 1. Q correlations between therapist's sort at 24 contacts and earlier sorts. (Phenotypic pool; $N = 182$ items; Stopol)

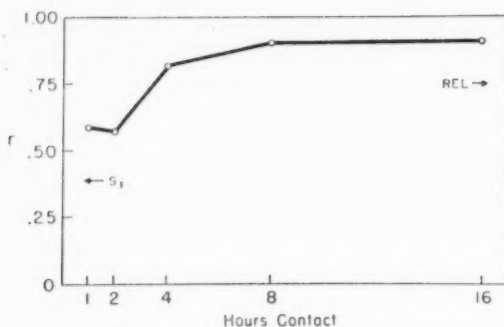


FIG. 2. Q correlations between therapist's sort at 24 contacts and earlier sorts. (Genotypic pool; $N=113$ items; Stopol)

of 24 hours is "the criterion," which would involve a concept of validation that I reject (Cronbach and Meehl, 1955, pp. 284-285, 292-294). But presumably his perception after 24 contacts is more trustworthy than after only one. Or, if we (a) assume that some information gained early is subsequently lost by forgetting, erroneous revisions, and the like; (b) take as our standard of comparison the average value of ratings over all six sortings; and (c) treat this as a kind of "best combined image," the essential character of the situation remains as shown.

Now this state of affairs presents any psychological test with a difficult task. If, after two to four hours of therapeutic interviewing, the therapist tends to arrive at a stable image of the patient which is not very different from the one he will have after 24 contacts, and if that final image is pretty accurate, the test would need to have very high validity before we could justify the expenditure of skilled psychological time in giving, scoring, interpreting, and communicating it.

When we first began this convergence study, our primary interest was in the pragmatic utility of the MMPI. One way to consider validity (which makes more practical sense than the conventional validation study) is to ask: "How long does it take the psychotherapist to find out what the test would have told him in the first place?" We were interested in plotting the Q correlation between a blind MMPI description of the patient and the successive sorts done by the therapist as he gathered more extensive samples of the latter's behavior during treatment, hoping to find that, as the therapist gets "wised up" by further interviews, he learns what

the MMPI would have told him all along. This pleasant fantasy was disturbed by the rapidity with which the therapist's image of the patient converges, even before the Campbell-Fiske correlations were run. It is of some interest to plot the curve of Q correlation between a "good" blind MMPI description of the patient and the successive descriptions by the therapist (Figure 3). These results are surely nothing to write home about!

In a recent paper reporting on an empirical study of MMPI sorting behavior (Meehl 1959b) I listed six factors or circumstances which might be expected theoretically to favor the clinician's brain as a cognizing and decision making instrument in competition with the traditional statistical methods of combining data. Among these six factors is one in which I have a particular interest, I suppose partly because it lends itself more readily to quantitative study than do some of the others. This factor is the presumed ability of the clinician to react on the basis of higher order configural relations (Meehl 1954, pp. 130-134; Horst 1954) by virtue of the fact that a system of variables can be graphically represented as a profile; and thereafter, given extensive clinical experience with a particular instrument, the clinician can respond to the visual gestalt. This he could do by *exemplifying* a complex mathematical function which neither he nor anyone else had as yet succeeded in *formulating*. The search for that function could take place in the context of studying the generalization and discrimination of complex visual forms. I recommend to your attention the recent work of Paul J. Hoffman on this subject, some of which has been reported (1958a, 1958b, 1959). Hoffman has undertaken a mathematical analysis of the rating behavior of judges who are presented with

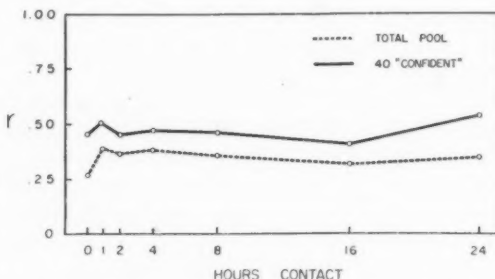


FIG. 3. Q correlations between MMPI reader's sort and successive sorts by therapist. (Phenotypic pool; Meehl and Stopol)

TABLE 1
CONCURRENT VALIDITY OF MEEHL-DAHLSTROM RULES
IN EIGHT CROSS-VALIDATION SAMPLES

Sample	N	H%	M%	I%	$\frac{H}{H+M}$	P
A*	92	55	16	28	.77	<.001
B*	77	45	29	26	.61	<.05
C	103	49	16	35	.75	<.001
D	42	40	21	38	.65	nonsig.
E*	181	45	18	36	.71	<.001
F	166	47	20	33	.70	<.001
G	273	63	12	25	.84	<.001
K*	54	78	5	17	.93	no test
Total	988	53	17	30	.76	.001

* Essentially uncontaminated samples.

multivariable profiles, and the application of his formulas should teach us a great deal about the clinician's cognitive activity.

Comparing the impressionistic judgment of a group of Minnesota clinicians as to the amount of "psychotic tendency" revealed by MMPI profiles with six statistical methods of treating the profiles, I found that the pooled judgment of 21 clinicians was significantly better (against the diagnostic criterion) than the linear discriminant function. In fact, there was a significant tendency (although slight) for even the *individual* clinicians to do a better job than the linear discriminant function. However, the best cross-validated results displayed by any method of sorting these profiles thus far tried utilizes a very complex set of configural rules developed by Grant Dahlstrom and myself (Meehl & Dahlstrom, 1960). Table 1 shows the results of applying these rules to almost a thousand cases from eight clinics over the United States. These rules were concocted by a combination of clinical experience with statistical checking; and, while relatively crude and surely failing to extract all of the profile information, they are more efficient at this than a linear combination of scores, the pooled judgments of 29 MMPI readers, or the judgment of the best of 29. Without knowing the form and constants of the mathematical function relating probability of psychosis to the MMPI variables, we cannot answer the question: "How much of the information contained in the profile is extracted by the clinician?" One may plot probability of psychosis as a function of the clinicians' placement of profiles on an 11-step subjective scale

of degree (or confidence) of psychoticism. Figure 4 shows probability of psychosis as a function of impressionistic profile placement by the best and worst clinician, and the pooled judgment of a group of 29. Figure 5 shows hit rate (whether neurotic or psychotic) as a function of the amount of consensus among 29 judges.

While our data do indicate that the clinician's judging behavior with respect to the psychoticism variable is significantly configural, the *amount* of departure from a linear, additive model does not appear to be very great. For many years, skeptical statisticians have been pointing out to us clinicians that there is more conversation about nonlinear functions than there is actual demonstration of such and, anyway, that the value of departures from linearity and additivity involved in clinical judgments is likely to be attenuated, if not completely washed out, by the clinician's assignment of nonoptimal weights and the unreliability invariably involved in the impressionistic use of multivariate data.

Lykken, Hoffman, and I plan to utilize some of the MMPI psychoticism data for the kinds of analysis the latter has suggested, but in the meantime I have applied one of Hoffman's formulas to a

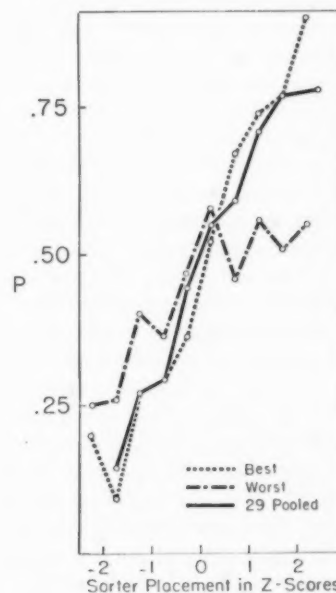


FIG. 4. Probability of psychosis as function of MMPI profile placement by sorters.

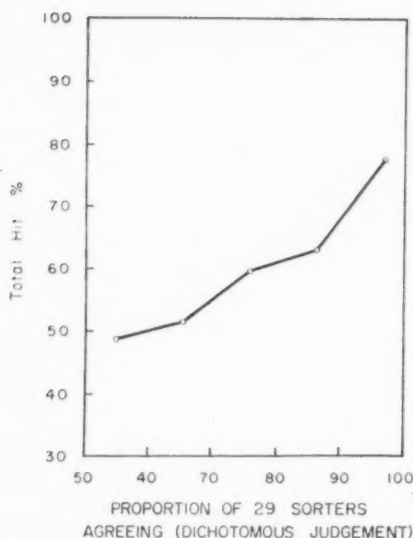


FIG. 5. Hit rate as function of MMPI sorter consensus. (neurosis-psychosis)

portion of these data. He suggests that, if we treat the clinician's quantitative sorting as the dependent variable, the multiple R of this variable upon the profile scores should differ from unity only because of the clinician's unreliability, provided his sorting behavior follows a linear model. The multiple R of the 11-step psychoticism ratings for my four best clinicians, when divided by the square root of their reliabilities (Hoffman's "complexity" formula), varies from .871 to .975, with a mean of .942, indicating that the departure of their judging behavior from a linear model is small. It is also interesting that the *intersorter* reliability (Horst's generalized coefficient) reaches .994 for the four best sorters and .987 for the four worst. Whatever these MMPI readers are doing when asked to judge psychoticism from the profile, they seem to be doing it in very much the same way.

Let me turn next to a brief account of an exploratory study which was a dismal failure and which I am still trying to figure out. All told, there now exist almost 200 different scoring keys for the MMPI item pool, ranging all the way from "dependency" to "baseball talent" and derived by a variety of methods (including factor analysis, face validity, and criterion keying). I thought it might be interesting to treat the patient's MMPI behavior more like the psychoanalyst than like the factor

analyst: namely, to overdetermine the psychology of the patient by scoring him on a large number of these scales, in spite of their redundancy. Imagine two patients who produce identical profiles when scored on a very large number of partially overlapping but distinguishable variables. One might hope, except for the intrinsic defects of *coverage* in the MMPI item pool, that such a pair of individuals would be, so to speak, pinpointed in personality space as very close together. In practice it is impossible to find identical (or even nearly identical) profiles as the number of scored dimensions is increased, but perhaps one could get an estimate of this extreme by extrapolating interpatient similarities from lesser degrees of profile resemblance.

Selecting a sample of 20 females outpatients rated by staff psychiatrists or psychologists in connection with a study on the new ataraxic Mellaril (Fleeson, Glueck, Heistad, King, Lykken, Meehl, & Mena, 1958), we calculated the interviewer rating Q correlations for all possible pairs, thus generating an interpatient resemblance matrix of 190 elements. Turning then to the MMPI (by which the clinical raters were, of course, uncontaminated) and eliminating scales of fewer than 10 or more than 80 items, we set up random sets of 10 scales after defining the first set of 10 as the basic profile of clinical scales commonly used. The Cronbach-Gleser distance measure was then computed on the MMPI profiles for the same 190 pairs. Thus we had a matrix of interpatient resemblances as clinically described by skilled interviewers through Q sorts and a corresponding matrix of MMPI profile similarity indices. A series of matrices of this latter kind was then generated by progressively extending the profile, adding successive blocks of 10 randomly chosen scales. Thus, the first MMPI matrix was based upon the interpatient distance measures for the usual 10 scores, the second one upon 20 scores (the usual 10 plus 10 randomly chosen), the third one on 30 scores, and so forth up to a profile of 160 variables! The idea, of course, was that through this procedure we would be squeezing all of the blood out of the psychometric turnip and that a second order correlation (apologies to the statisticians) between the corresponding elements of the two matrices would show a steady rise.

It would have been very nice had the asymptote of this intermatrix coefficient, when plotted as a function of the number of MMPI variables enter-

ing into the distance measure, approached a very high value. That is, if you measure—however unreliably and redundantly—a huge mass of variables (schizoid trend, recidivism, dominance, defensiveness, baseball talent, dependency, control, ego strength, use of repression, tendency to homesickness, academic potential, etc.), then the psychological resemblance between two patients will be closely related to their profile similarity on this extended list of MMPI scores. It turned out that there was no problem of curve fitting, for the simple reason that the intermatrix resemblances began at zero for the first 10 scales and remained at zero, without the slightest tendency to increase as we included further blocks of scales in computing the distance measures. We know from a good deal of evidence that neither the MMPI nor the clinical Q sorts are quite *that* bad, and I am at a loss to understand these results. My suspicion is that they arise from inadequacies of the distance measure itself, and further analysis of the data is being undertaken with this hypothesis in mind. I still think that it was an interesting idea.

Leaving profile pattern interpretation, I should like to consider one more topic briefly. One of the most important problems in clinical psychology is deciding what kind of language communicates the largest amount of information about a patient. Most clinical practice today is predicated upon the assumption that useful statements about the patient can best be formulated (or at least inferentially mediated) by a theoretical language. The power of theoretical discourse in the other sciences makes this predilection understandable, and the characteristic Allport-Vernon-Lindzey profiles of clinical psychologists reflect strong theoretical interest. However, we learn in undergraduate physics that in order to apply theoretical constructs to the solution of practical problems (specifically, to predict the subsequent course of a particular physical system), one must fulfill two conditions. First, he must possess a reasonably well developed theory. That is, he must know the laws that systems of the given kind obey. Secondly, he must have a technology, a set of measuring instruments, for determining the initial and boundary conditions of the particular system under study. To the extent that either, or both, of these conditions are not fulfilled, predictions arrived at by theoretical inference will be untrustworthy. I do not see how anyone taking an objective view of the enterprise

could claim that we fulfill *either*, let alone both, of these conditions in clinical psychology today. For this reason, in spite of my own personal interest in theoretical questions, I remain among that minority who persist in skepticism as to the pragmatic utility of theoretical constructions in daily clinical decision making.

Suppose, however, that some kind of theoretical discourse is to be used; which of the several kinds of theoretical sublanguages is most economical? As a pilot study in connection with a Ford Foundation project now going on at Minnesota, I collected some preliminary data which you may find of interest. Twenty psychotherapists were asked to describe a patient whom they had had in treatment for at least 25 hours, using the 182-item phenotypic pool which generated the curves previously shown. They also described the patient in terms of the 113-item genotypic pool. Although the latter pool was not constructed in any systematic way with respect to theoretical orientation, having been built for a different purpose, one can identify five relatively homogeneous subsets of genotypic items as follows: 25 Murray needs, 14 areas of conflict, 13 mechanisms of defense, 10 value-orientation components, and 7 items referring to dimensions of psychiatric nosology. After calculating the 190 interpatient Q correlations based upon each of these subpools, we may ask how well the pattern of interpatient resemblances in the phenotype is reproduced by the genotypic matrix. Unfortunately, I have not been able to find a statistician who will tell me how to do a significance test on such data, but the coefficients obtained are shown in Table 2. It is remarkable, I think, that the 13 defense mechanisms do about as well in reproducing the 182-item phenotypic matrix as does the entire genotypic pool consisting of almost 10 times as many items. We hope that with a more systematic coverage of the domain the Ford project will give us some definite information about this question.

I have presented some samples of research currently in progress at Minnesota which, while somewhat heterogeneous and difficult to pull together, all treat of what we see as pragmatically important aspects of the clinician's cognitive activity. In order to place any confidence in either the theoretical constructs we employ in discussing patients, or in the instrument-interpreter combinations we use to assess them, studies of convergent and discrim-

TABLE 2
CORRELATIONS BETWEEN INTERPATIENT *P* MATRIX
AND *G* MATRICES BASED ON VARIOUS
SUBPOOLS

Variables	<i>r</i>
P (182 items) vs. entire G pool (113 items)	.59
P vs. 13 defense mechanisms	.52
P vs. 25 Murray needs	.22
P vs. 7 nosological components	.22
P vs. 10 value dimensions	.03
P vs. 14 conflict areas	-.03
P vs. all 69 G items in above subpools	.45

Note.—²⁰C₂ patients rated; *N* = 190 coefficients.

inative validity must be carried out. The Campbell-Fiske multitrait-multimethod matrix, or the multiperson-multimethod variant of it, should be useful for this purpose. It seems obvious that even adequate and sophisticated studies of construct validity must be supplemented by data upon the rate at which the clinician acquires information from various sources. Since the commonest justification for expenditure of psychometric time is the utility to the therapist of "advance knowledge" (especially of the genotype), the skepticism expressed by our sample of psychotherapists, taken in combination with the convergence curves for the therapist's perception of his patient, put this widely held belief badly in need of experimental support. An important aspect of such data, presumably rather specific to various populations and clinical instruments, is that of differential convergence rates among items. There are probably certain attributes for which a test's validity is insufficient to justify a marked departure from the base rates or mean rating of the given clinical population, and others for which the therapist tends to be in error early in the game and to converge to the truth rather slowly in contrast to the test. I would predict that an example of this is MMPI Scale 6, which is a rather weak scale when used as an exclusion test, but which, when elevated, turns out almost invariably to be right. I have had patients in treatment whose paranoid potential did not manifest itself until 50 or 75 sessions, by which time I had concluded (erroneously) that the MMPI was giving me a false positive.

As has been pointed out by many clinicians, lacking adequate clinical cookbooks (Meehl 1956) we

have in practice to treat our instruments as instrument-interpreter combinations. I believe we can say upon present evidence that no one interpreter succeeds in extracting all of the information contained in a profile and that the development of objective configural methods of profile analysis (of which the Meehl-Dahlstrom rules are a primitive example) is a task of great importance. David Lykken and I are currently engaged in a study comparing more complex functions—such as a second degree polynomial having squares and cross-products—with clinical judgment and the Meehl-Dahlstrom Rules. I am betting on the last-named, because—while nonoptimally weighted—they do at least tap configural effects involving interactions up to the sixth order.

Finally, the question of what is the most economical language to employ in describing a patient remains open, although it appears that there are many practitioners who are not sufficiently aware that this problem exists.

I look forward to the next decade of research in clinical psychology with a certain ambivalence. We are asking more sensible questions and being more critical of our procedures; and several research techniques are now available, and in wide use, which should give us some pretty clear answers. The reason for my ambivalence (and I regret that in the role of prophet I have to sound like Jeremiah) is that the evidence already available suggests that the outcomes will look pretty gloomy. My advice to fledgling clinical psychologists is to construct their self-concept mainly around "I am a researcher" or "I am a psychotherapist," because one whose self-concept is mainly "I am a (test oriented) psychodiagnostician" may have to maintain his professional security over the next few years by not reading the research literature, a maneuver which has apparently proved quite successful already for some clinicians. Personally, I find the cultural lag between what the published research shows and what clinicians persist in claiming to do with their favorite devices even more disheartening than the adverse evidence itself.

Psychologists cannot administer shock treatment or pass out tranquilizers, and I do not know of any evidence that we are better psychotherapists than our psychiatric colleagues. If there is anything that justifies our existence—other than the fact that we come cheaper—it is that we think scientifically about human behavior and that we come

from a long tradition, going way back to the very origins of experimental psychology in the study of human error, of being critical of ourselves as cognizing organisms and of applying quantitative methods to the outcomes of our cognitive activity. If this methodological commitment is not strong enough to compete with the commitments clinicians have to particular diagnostic instruments, the unique contribution of our discipline will have been lost. I can mobilize some enthusiasm for the next 10 years within the field: while I expect discouraging findings at the level of practice, from the standpoint of the sociology of professions and the history of ideas, the developments should be very interesting to watch.

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PIGEONS IN A PELICAN

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THIS is the history of a crackpot idea, born on the wrong side of the tracks intellectually speaking, but eventually vindicated in a sort of middle class respectability. It is the story of a proposal to use living organisms to guide missiles—of a research program during World War II called "Project Pigeon" and a peacetime continuation at the Naval Research Laboratory called "ORCON," from the words "organic control." Both of these programs have now been declassified.

Man has always made use of the sensory capacities of animals, either because they are more acute than his own or more convenient. The watchdog probably hears better than his master and in any case listens while his master sleeps. As a detecting system the dog's ear comes supplied with an alarm (the dog need not be taught to announce the presence of an intruder), but special forms of reporting are sometimes set up. The tracking behavior of the bloodhound and the pointing of the hunting dog are usually modified to make them more useful. Training is sometimes quite explicit. It is said that sea gulls were used to detect submarines in the English Channel during World War I. The British sent their own submarines through the Channel releasing food to the surface. Gulls could see the submarines from the air and learned to follow them, whether they were British or German. A flock of gulls, spotted from the shore, took on special significance. In the seeing-eye dog the repertoire of artificial signaling responses is so elaborate that it has the conventional character of the verbal interchange between man and man.

The detecting and signaling systems of lower organisms have a special advantage when used with explosive devices which can be guided toward the objects they are to destroy, whether by land, sea, or air. Homing systems for guided missiles have now been developed which sense and signal the position of a target by responding to visible or invisible radiation, noise, radar reflections, and so on. These have not always been available, and in any case a living organism has certain advan-

tages. It is almost certainly cheaper and more compact and, in particular, is especially good at responding to patterns and those classes of patterns called "concepts." The lower organism is not used because it is more sensitive than man—after all, the kamikaze did very well—but because it is readily expendable.

PROJECT PELICAN

The ethical question of our right to convert a lower creature into an unwitting hero is a peacetime luxury. There were bigger questions to be answered in the late thirties. A group of men had come into power who promised, and eventually accomplished, the greatest mass murder in history. In 1939 the city of Warsaw was laid waste in an unprovoked bombing, and the airplane emerged as a new and horrible instrument of war against which only the feeblest defenses were available. Project Pigeon was conceived against that background. It began as a search for a homing device to be used in a surface-to-air guided missile as a defense against aircraft. As the balance between offensive and defensive weapons shifted, the direction was reversed, and the system was to be tested first in an air-to-ground missile called the "Pelican." Its name is a useful reminder of the state of the missile art in America at that time. Its detecting and servomechanisms took up so much space that there was no room for explosives: hence the resemblance to the pelican "whose beak can hold more than its belly can." My title is perhaps now clear. Figure 1 shows the pigeons, jacketed for duty. Figure 2 shows the beak of the Pelican.

At the University of Minnesota in the spring of 1940 the capacity of the pigeon to steer toward a target was tested with a moving hoist. The pigeon, held in a jacket and harnessed to a block, was immobilized except for its neck and head. It could eat grain from a dish and operate a control system by moving its head in appropriate directions. Movement of the head operated the motors of the hoist. The bird could ascend by lifting its head, descend by lowering it, and travel from side to side

by moving appropriately. The whole system, mounted on wheels, was pushed across a room toward a bull's-eye on the far wall. During the approach the pigeon raised or lowered itself and moved from side to side in such a way as to reach the wall in position to eat grain from the center of the bull's-eye. The pigeon learned to reach any target within reach of the hoist, no matter what the starting position and during fairly rapid approaches.

The experiment was shown to John T. Tate, a physicist, then Dean of the Graduate School at the University of Minnesota, who brought it to the attention of R. C. Tolman, one of a group of scientists engaged in early defense activities. The result was the first of a long series of rejections. The proposal "did not warrant further development at the time." The project was accordingly allowed to lapse. On December 7, 1941 the situation was suddenly restructured; and, on the following day, with the help of Keller Breland, then a graduate student at Minnesota, further work was planned. A simpler harnessing system could be used if the bomb were to rotate slowly during its descent, when the pigeon would need to steer in only one dimension: from side to side. We built an apparatus in which a harnessed pigeon was lowered toward a large revolving turntable across which a target was driven according to contacts made by the bird during its descent. It was not difficult to train a pigeon to "hit" small ship models during fairly rapid descents. We made a demonstration film showing hits on various kinds of targets, and two psychologists then engaged in the war effort in Washington, Charles Bray and Leonard Carmichael, undertook to look for government support. Tolman, then at the Office of Scientific Research and

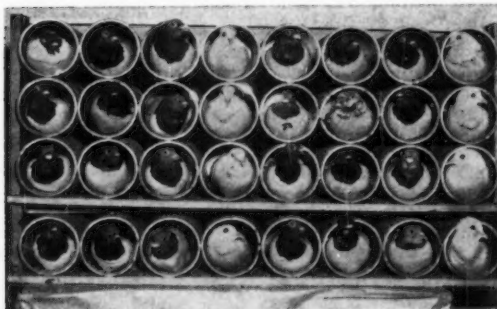


FIG. 1. Thirty-two pigeons, jacketed for testing.

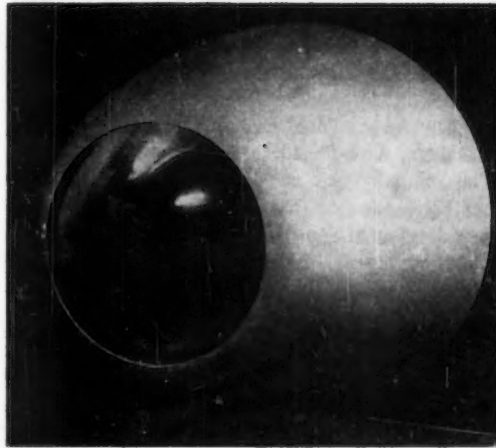


FIG. 2. Nose of the Pelican, showing lenses.

Development, again felt that the project did not warrant support, in part because the United States had at that time no missile capable of being guided toward a target. Commander (now Admiral) Luis de Florez, then in the Special Devices Section of the Navy, took a sympathetic view. He dismissed the objection that there was no available vehicle by suggesting that the pigeon be connected with an automatic pilot mounted in a small plane loaded with explosives. But he was unable to take on the project because of other commitments and because, as he explained, he had recently bet on one or two other equally long shots which had not come in.

The project lapsed again and would probably have been abandoned if it had not been for a young man whose last name I have ungratefully forgotten, but whose first name—Victor—we hailed as a propitious sign. His subsequent history led us to refer to him as Vanquished; and this, as it turned out, was a more reliable omen. Victor walked into the Department of Psychology at Minnesota one day in the summer of 1942 looking for an animal psychologist. He had a scheme for installing dogs in antisubmarine torpedoes. The dogs were to respond to faint acoustic signals from the submarine and to steer the torpedo toward its goal. He wanted a statement from an animal psychologist as to its feasibility. He was understandably surprised to learn of our work with pigeons but seized upon it eagerly, and citing it in support of his contention that dogs could be trained to steer torpedoes he went to a number of com-

panies in Minneapolis. His project was rejected by everyone he approached; but one company, General Mills, Inc., asked for more information about our work with pigeons. We described the project and presented the available data to Arthur D. Hyde, Vice-President in Charge of Research. The company was not looking for new products, but Hyde thought that it might, as a public service, develop the pigeon system to the point at which a governmental agency could be persuaded to take over.

Breland and I moved into the top floor of a flour mill in Minneapolis and with the help of Norman Guttman, who had joined the project, set to work on further improvements. It had been difficult to induce the pigeon to respond to the small angular displacement of a distant target. It would start working dangerously late in the descent. Its natural pursuit behavior was not appropriate to the characteristics of a likely missile. A new system was therefore designed. An image of the target was projected on a translucent screen as in a camera obscura. The pigeon, held near the screen, was reinforced for pecking at the image on the screen. The guiding signal was to be picked up from the point of contact of screen and beak.

In an early arrangement the screen was a translucent plastic plate forming the larger end of a truncated cone bearing a lens at the smaller end. The cone was mounted, lens down, in a gimbal bearing. An object within range threw its image on the translucent screen; and the pigeon, held vertically just above the plate, pecked the image. When a target was moved about within range of the lens, the cone continued to point to it. In another apparatus a translucent disk, free to tilt slightly on gimbal bearings, closed contacts operating motors which altered the position of a large field beneath the apparatus. Small cutouts of ships and other objects were placed on the field. The field was constantly in motion, and a target would go out of range unless the pigeon continued to control it. With this apparatus we began to study the pigeon's reactions to various patterns and to develop sustained steady rates of responding through the use of appropriate schedules of reinforcement, the reinforcement being a few grains occasionally released onto the plate. By building up large extinction curves a target could be tracked continuously for a matter of minutes without reinforcement. We trained pigeons to follow a variety of

land and sea targets, to neglect large patches intended to represent clouds or flak, to concentrate on one target while another was in view, and so on. We found that a pigeon could hold the missile on a particular street intersection in an aerial map of a city. The map which came most easily to hand was of a city which, in the interests of international relations, need not be identified. Through appropriate schedules of reinforcement it was possible to maintain longer uninterrupted runs than could conceivably be required by a missile.

We also undertook a more serious study of the pigeon's behavior, with the help of W. K. Estes and Marion Breland who joined the project at this time. We ascertained optimal conditions of deprivation, investigated other kinds of deprivations, studied the effect of special reinforcements (for example, pigeons were said to find hemp seed particularly delectable), tested the effects of energizing drugs and increased oxygen pressures, and so on. We differentially reinforced the force of the pecking response and found that pigeons could be induced to peck so energetically that the base of the beak became inflamed. We investigated the effects of extremes of temperature, of changes in atmospheric pressure, of accelerations produced by an improvised centrifuge, of increased carbon dioxide pressure, of increased and prolonged vibration, and of noises such as pistol shots. (The birds could, of course, have been deafened to eliminate auditory distractions, but we found it easy to maintain steady behavior in spite of intense noises and many other distracting conditions using the simple process of adaptation.) We investigated optimal conditions for the quick development of discriminations and began to study the pigeon's reactions to patterns, testing for induction from a test figure to the same figure inverted, to figures of different sizes and colors, and to figures against different grounds. A simple device using carbon paper to record the points at which a pigeon pecks a figure showed a promise which has never been properly exploited.

We made another demonstration film and renewed our contact with the Office of Scientific Research and Development. An observer was sent to Minneapolis, and on the strength of his report we were given an opportunity to present our case in Washington in February 1943. At that time we were offering a homing device capable of reporting with an on-off signal the orientation of a missile

toward various visual patterns. The capacity to respond to pattern was, we felt, our strongest argument, but the fact that the device used only visible radiation (the same form of information available to the human bombardier) made it superior to the radio controlled missiles then under development because it was resistant to jamming. Our film had some effect. Other observers were sent to Minneapolis to see the demonstration itself. The pigeons, as usual, behaved beautifully. One of them held the supposed missile on a particular intersection of streets in the aerial map for five minutes although the target would have been lost if the pigeon had paused for a second or two. The observers returned to Washington, and two weeks later we were asked to supply data on (a) the population of pigeons in the United States (fortunately, the census bureau had some figures) and (b) the accuracy with which pigeons struck a point on a plate. There were many arbitrary conditions to be taken into account in measuring the latter, but we supplied possibly relevant data. At long last, in June 1943, the Office of Scientific Research and Development awarded a modest contract to General Mills, Inc. to "develop a homing device."

At that time we were given some information about the missile the pigeons were to steer. The Pelican was a wing steered glider, still under development and not yet successfully steered by any homing device. It was being tested on a target in New Jersey consisting of a stirrup shaped pattern bulldozed out of the sandy soil near the coast. The white lines of the target stood out clearly against brown and green cover. Colored photographs were taken from various distances and at various angles, and the verisimilitude of the reproduction was checked by flying over the target and looking at its image in a portable camera obscura.

Because of security restrictions we were given only very rough specifications of the signal to be supplied to the controlling system in the Pelican. It was no longer to be simply on-off; if the missile was badly off target, an especially strong correcting signal was needed. This meant that the quadrant-contact system would no longer suffice. But further requirements were left mainly to our imagination. The General Mills engineers were equal to this difficult assignment. With what now seems like unbelievable speed, they designed and constructed a pneumatic pickup system giving a graded signal.

A lens in the nose of the missile threw an image on a translucent plate within reach of the pigeon in a pressure sealed chamber. Four air valves resting against the edges of the plate were jarred open momentarily as the pigeon pecked. The valves at the right and left admitted air to chambers on opposite sides of one tambour, while the valves at the top and bottom admitted air to opposite sides of another. Air on all sides was exhausted by a Venturi cone on the side of the missile. When the missile was on target, the pigeon pecked the center of the plate, all valves admitted equal amounts of air, and the tambours remained in neutral positions. But if the image moved as little as a quarter of an inch off-center, corresponding to a very small angular displacement of the target, more air was admitted by the valves on one side, and the resulting displacement of the tambours sent appropriate correcting orders directly to the servosystem.

The device required no materials in short supply, was relatively foolproof, and delivered a graded signal. It had another advantage. By this time we had begun to realize that a pigeon was more easily controlled than a physical scientist serving on a committee. It was very difficult to convince the latter that the former was an orderly system. We therefore multiplied the probability of success by designing a multiple bird unit. There was adequate space in the nose of the Pelican for three pigeons each with its own lens and plate. A net signal could easily be generated. The majority vote of three pigeons offered an excellent guarantee against momentary pauses and aberrations. (We later worked out a system in which the majority took on a more characteristically democratic function. When a missile is falling toward *two* ships at sea, for example, there is no guarantee that all three pigeons will steer toward the same ship. But at least two must agree, and the third can then be punished for his minority opinion. Under proper contingencies of reinforcement a punished bird will shift immediately to the majority view. When all three are working on one ship, any defection is immediately punished and corrected.)

The arrangement in the nose of the Pelican is shown in Figure 3. Three systems of lenses and mirrors, shown at the left, throw images of the target area on the three translucent plates shown in the center. The ballistic valves resting against the edges of these plates and the tubes connecting them with the manifolds leading to the controlling

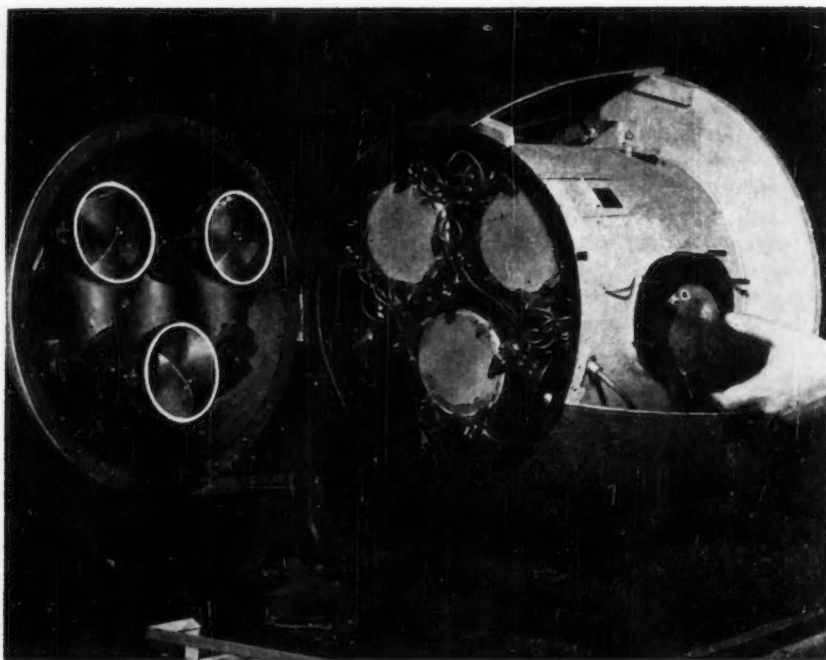


FIG. 3. Demonstration model of the three-pigeon guidance system.

tambours may be seen. A pigeon is being placed in the pressurized chamber at the right.

The General Mills engineers also built a simulator (Figure 4)—a sort of Link trainer for pigeons—designed to have the steering characteristics of the Pelican, in so far as these had been communicated to us. Like the wing steered Pelican, the simulator tilted and turned from side to side.

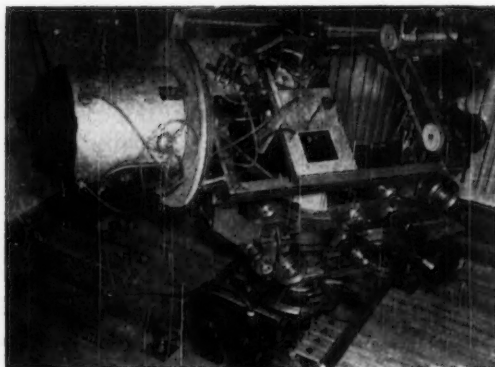


FIG. 4. Simulator for testing the adequacy of the pigeon signal.

When the three-bird nose was attached to it, the pigeons could be put in full control—the “loop could be closed”—and the adequacy of the signal tested under pursuit conditions. Targets were moved back and forth across the far wall of a room at prescribed speeds and in given patterns of oscillation, and the tracking response of the whole unit was studied quantitatively.

Meanwhile we continued our intensive study of the behavior of the pigeon. Looking ahead to combat use we designed methods for the mass production of trained birds and for handling large groups of trained subjects. We were proposing to train certain birds for certain *classes* of targets, such as ships at sea, while special squads were to be trained on special targets, photographs of which were to be obtained through reconnaissance. A large crew of pigeons would then be waiting for assignment; but we developed harnessing and training techniques which should have solved such problems quite easily.

A multiple unit trainer is shown in Figure 5. Each box contains a jacketed pigeon held at an angle of 45° to the horizontal and perpendicular

to an 8" x 8" translucent screen. A target area is projected on each screen. Two beams of light intersect at the point to be struck. All on-target responses of the pigeon are reported by the interruption of the crossed beams and by contact with the translucent screen. Only a four-inch, disk shaped portion of the field is visible to the pigeon at any time, but the boxes move slowly about the field, giving the pigeon an opportunity to respond to the target in all positions. The positions of all reinforcements are recorded to reveal any weak areas. A variable-ratio schedule is used to build sustained, rapid responding.

By December 1943, less than six months after the contract was awarded, we were ready to report to the Office of Scientific Research and Development. Observers visited the laboratory and watched the simulator follow a target about a room under the control of a team of three birds. They also reviewed our tracking data. The only questions which arose were the inevitable consequence of our lack of information about the signal required to steer the Pelican. For example, we had had to make certain arbitrary decisions in compromising between sensitivity of signal and its integration or smoothness. A high vacuum produced quick, rather erratic movements of the tambours, while a lower vacuum gave a sluggish but smooth signal. As it turned out, we had not chosen the best values in collecting our data, and in January 1944 the Office of Scientific Research and Development refused to extend the General Mills contract. The reasons given seemed to be due to misunderstandings or, rather, to lack of communication. We had already collected further data with new settings of the instruments, and these were submitted in a request for reconsideration.

We were given one more chance. We took our new data to the radiation lab at the Massachusetts Institute of Technology where they were examined by the servospecialists working on the Pelican controls. To our surprise the scientist whose task it was to predict the usefulness of the pigeon signal argued that our data were inconsistent with respect to phase lag and certain other characteristics of the signal. According to his equations, our device could not possibly yield the signals we reported. We knew, of course, that it had done so. We examined the supposed inconsistency and traced it, or so we thought, to a certain nonlinearity in our system. In pecking an image near the edge of the

plate, the pigeon strikes a more glancing blow; hence the air admitted at the valves is not linearly proportional to the displacement of the target. This could be corrected in several ways: for example, by using a lens to distort radial distances. It was our understanding that in any case the signal was adequate to control the Pelican. Indeed, one servo authority, upon looking at graphs of the performance of the simulator, exclaimed: "This is better than radar!"

Two days later, encouraged by our meeting at MIT, we reached the summit. We were to present our case briefly to a committee of the country's top scientists. The hearing began with a brief report by the scientist who had discovered the "inconsistency" in our data, and to our surprise he still regarded it as unresolved. He predicted that the signal we reported would cause the missile to "hunt" wildly and lose the target. But his prediction should have applied as well to the closed loop simulator. Fortunately another scientist was present who had seen the simulator performing under excellent control and who could confirm our report of the facts. But reality was no match for mathematics.

The basic difficulty, of course, lay in convincing a dozen distinguished physical scientists that the behavior of a pigeon could be adequately controlled. We had hoped to score on this point by bringing with us a demonstration. A small black box had a round translucent window in one end. A slide projector placed some distance away threw on the window an image of the New Jersey target. In the box, of course, was a pigeon—which, incidentally, had at that time been harnessed for 35 hours. Our intention was to let each member of the committee observe the response to the target by



FIG. 5. A trainer for four pigeons.

looking down a small tube; but time was not available for individual observation, and we were asked to take the top off the box. The translucent screen was flooded with so much light that the target was barely visible, and the peering scientists offered conditions much more unfamiliar and threatening than those likely to be encountered in a missile. In spite of this the pigeon behaved perfectly, pecking steadily and energetically at the image of the target as it moved about on the plate. One scientist with an experimental turn of mind intercepted the beam from the projector. The pigeon stopped instantly. When the image again appeared, pecking began within a fraction of a second and continued at a steady rate.

It was a perfect performance, but it had just the wrong effect. One can talk about phase lag in pursuit behavior and discuss mathematical predictions of hunting without reflecting too closely upon what is inside the black box. But the spectacle of a living pigeon carrying out its assignment, no matter how beautifully, simply reminded the committee of how utterly fantastic our proposal was. I will not say that the meeting was marked by unrestrained merriment, for the merriment was restrained. But it was there, and it was obvious that our case was lost.

Hyde closed our presentation with a brief summary: we were offering a homing device, unusually resistant to jamming, capable of reacting to a wide variety of target patterns, requiring no materials in short supply, and so simple to build that production could be started in 30 days. He thanked the committee, and we left. As the door closed behind us, he said to me: "Why don't you go out and get drunk!"

Official word soon came: "Further prosecution of this project would seriously delay others which in the minds of the Division would have more immediate promise of combat application." Possibly the reference was to a particular combat application at Hiroshima a year and a half later, when it looked for a while as if the need for accurate bombing had been eliminated for all time. In any case we had to show, for all our trouble, only a loftful of curiously useless equipment and a few dozen pigeons with a strange interest in a feature of the New Jersey coast. The equipment was scrapped, but 30 of the pigeons were kept to see how long they would retain the appropriate behavior.

In the years which followed there were faint signs of life. Winston Churchill's personal scientific advisor, Lord Cherwell, learned of the project and "regretted its demise." A scientist who had had some contact with the project during the war, and who evidently assumed that its classified status was not to be taken seriously, made a good story out of it for the *Atlantic Monthly*, names being changed to protect the innocent. Other uses of animals began to be described. The author of the *Atlantic Monthly* story also published an account of the "incendiary bats." Thousands of bats were to be released over an enemy city, each carrying a small incendiary time bomb. The bats would take refuge, as is their custom, under eaves and in other out-of-the-way places; and shortly afterwards thousands of small fires would break out practically simultaneously. The scheme was never used because it was feared that it would be mistaken for germ warfare and might lead to retaliation in kind.

Another story circulating at the time told how the Russians trained dogs to blow up tanks. I have described the technique elsewhere (Skinner, 1956). A Swedish proposal to use seals to achieve the same end with submarines was not successful. The seals were to be trained to approach submarines to obtain fish attached to the sides. They were then to be released carrying magnetic mines in the vicinity of hostile submarines. The required training was apparently never achieved. I cannot vouch for the authenticity of probably the most fantastic story of this sort, but it ought to be recorded. The Russians were said to have trained sea lions to cut mine cables. A complicated device attached to the sea lion included a motor driven cable-cutter, a tank full of small fish, and a device which released a few fish into a muzzle covering the sea lion's head. In order to eat, the sea lion had to find a mine cable and swim along side it so that the cutter was automatically triggered, at which point a few fish were released from the tank into the muzzle. When a given number of cables had been cut, both the energy of the cutting mechanism and the supply of fish were exhausted, and the sea lion received a special stimulus upon which it returned to its home base for special reinforcement and reloading.

ORCON

The story of our own venture has a happy ending. With the discovery of German accomplish-

ments in the field of guided missiles, feasible homing systems suddenly became very important. Franklin V. Taylor of the Naval Research Laboratory in Washington, D. C. heard about our project and asked for further details. As a psychologist Taylor appreciated the special capacity of living organisms to respond to visual patterns and was aware of recent advances in the control of behavior. More important, he was a skillful practitioner in a kind of control which our project had conspicuously lacked: he knew how to approach the people who determine the direction of research. He showed our demonstration film so often that it was completely worn out—but to good effect, for support was eventually found for a thorough investigation of "organic control" under the general title ORCON. Taylor also enlisted the support of engineers in obtaining a more effective report of the pigeon's behavior. The translucent plate upon which the image of the target was thrown had a semiconducting surface, and the tip of the bird's beak was covered with a gold electrode. A single contact with the plate sent an immediate report of the location of the target to the controlling mechanism. The work which went into this system contributed to the so-called Pick-off Display Converter developed as part of the Naval Data Handling System for human observers. It is no longer necessary for the radar operator to give a verbal report of the location of a pip on the screen. Like the pigeon, he has only to touch the pip with a special contact. (He holds the contact in his hand.)



FIG. 6. Arrangement for studying pursuit movements.



FIG. 7. Frames from a simulated approach.

At the Naval Research Laboratory in Washington the responses of pigeons were studied in detail. Average peck rate, average error rate, average hit rate, and so on were recorded under various conditions. The tracking behavior of the pigeon was analyzed with methods similar to those employed with human operators (Figure 6). Pattern perception was studied, including generalization from one pattern to another. A simulator was constructed in which the pigeon controlled an image projected by a moving-picture film of an actual target: for example, a ship at sea as seen from a plane approaching at 600 miles per hour. A few frames of a moving picture of the pigeon controlling the orientation toward a ship during an approach are shown in Figure 7.

The publications from the Naval Research Laboratory which report this work (Chernikoff & Newlin, 1951; Conklin, Newlin, Taylor, & Tipton,

1953; Searle & Stafford, 1950; Taylor, 1949; White, 1952) provide a serious evaluation of the possibilities of organic control. Although in simulated tests a single pigeon occasionally loses a target, its tracking characteristics are surprisingly good. A three- or seven-bird unit with the same individual consistency should yield a signal with a reliability which is at least of the order of magnitude shown by other phases of guided missiles in their present stage of development. Moreover, in the seven years which have followed the last of these reports, a great deal of relevant information has been acquired. The color vision of the pigeon is now thoroughly understood; its generalization along single properties of a stimulus has been recorded and analyzed; and the maintenance of behavior through scheduling of reinforcement has been drastically improved, particularly in the development of techniques for pacing responses for less erratic and steadier signals (Skinner, 1957). Tests made with the birds salvaged from the old Project Pigeon showed that even after six years of inactivity a pigeon will immediately and correctly strike a target to which it has been conditioned and will continue to respond for some time without reinforcement.

The use of living organisms in guiding missiles is, it seems fair to say, no longer a crackpot idea. A pigeon is an extraordinarily subtle and complex mechanism capable of performances which at the moment can be equalled by electronic equipment only of vastly greater weight and size, and it can be put to reliable use through the principles which have emerged from an experimental analysis of its behavior. But this vindication of our original proposal is perhaps the least important result. Something happened during the brief life of Project Pigeon which it has taken a long time to appreciate. The practical task before us created a new attitude toward the behavior of organisms. We had to maximize the probability that a given form of behavior would occur at a given time. We could not enjoy the luxury of observing one variable while allowing others to change in what we hoped was a random fashion. We had to discover all relevant variables and submit them to experimental control whenever possible. We were no doubt under exceptional pressure, but vigorous scientific research usually makes comparable demands. Psychologists have too often yielded to the temptation to be content with hypothetical processes and inter-

vening variables rather than press for rigorous experimental control. It is often intellectual laziness rather than necessity which recommends the *a posteriori* statistical treatment of variation. Our task forced us to emphasize prior experimental control, and its success in revealing orderly processes gave us an exciting glimpse of the superiority of laboratory practice over verbal (including some kinds of mathematical) explanation.

THE CRACKPOT IDEA

If I were to conclude that crackpot ideas are to be encouraged, I should probably be told that psychology has already had more than its share of them. If it has, they have been entertained by the wrong people. Reacting against the excesses of psychological quackery, psychologists have developed an enormous concern for scientific respectability. They constantly warn their students against questionable facts and unsupported theories. As a result the usual PhD thesis is a model of compulsive cautiousness, advancing only the most timid conclusions thoroughly hedged about with qualifications. But it is just the man capable of displaying such admirable caution who needs a touch of uncontrolled speculation. Possibly a generous exposure to psychological science fiction would help. Project Pigeon might be said to support that view. Except with respect to its avowed goal, it was, as I see it, highly productive; and this was in large measure because my colleagues and I knew that, in the eyes of the world, we were crazy.

One virtue in crackpot ideas is that they breed rapidly and their progeny show extraordinary mutations. Everyone is talking about teaching machines nowadays, but Sidney Pressey can tell you what it was like to have a crackpot idea in that field 40 years ago. His self-testing devices and self-scoring test forms now need no defense, and psychomotor training devices have also achieved a substantial respectability. This did not, however, prepare the way for devices to be used in verbal instruction—that is, in the kinds of teaching which are the principal concern of our schools and colleges. Even five short years ago that kind of instruction by machine was still in the crackpot category. (I can quote official opinion to that effect from high places.) Now, there is a direct genetic connection between teaching machines and

Project Pigeon. We had been forced to consider the mass education of pigeons. True, the scrap of wisdom we imparted to each was indeed small, but the required changes in behavior were similar to those which must be brought about in vaster quantities in human students. The techniques of shaping behavior and of bringing it under stimulus control which can be traced, as I have suggested elsewhere (Skinner, 1958), to a memorable episode on the top floor of that flour mill in Minneapolis needed only a detailed reformulation of verbal behavior to be directly applicable to education.

I am sure there is more to come. In the year which followed the termination of Project Pigeon I wrote *Walden Two* (Skinner, 1948), a utopian picture of a properly engineered society. Some psychotherapists might argue that I was suffering from personal rejection and simply retreated to a fantasied world where everything went according to plan, where there never was heard a discouraging word. But another explanation is, I think, equally plausible. That piece of science fiction was a declaration of confidence in a technology of behavior. Call it a crackpot idea if you will; it is one in which I have never lost faith. I still believe that

the same kind of wide-ranging speculation about human affairs, supported by studies of compensating rigor, will make a substantial contribution toward that world of the future in which, among other things, there will be no need for guided missiles.

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INTERNSHIPS FOR DOCTORAL TRAINING IN CLINICAL PSYCHOLOGY APPROVED BY THE AMERICAN PSYCHOLOGICAL ASSOCIATION

November 1, 1959

ON the recommendation of the Committee on Evaluation, the Education and Training Board with the concurrence of the Board of Directors of the American Psychological Association has approved the internships for doctoral training in clinical psychology which are offered by the agencies listed below. These internships meet at least the minimum standards stated in the *American Psychologist* for November 1950, Vol. 5, pp. 594-609. All these agencies provide supervised experience in the three activities of diagnostic work, psychotherapy, and research unless otherwise stated. The committee used the criterion that thorough practicum training in at least two of these activities was a minimum standard for approval.

This list is complete for those agencies which had been evaluated by November 1, 1959. Other agencies are being rapidly improved, so that additions

to this list are expected when it is published next year.

The list below includes only independent agencies, that is, those agencies accepting interns from more than one university. Captive agencies, that is, those agencies in which practicum training is available only to students of a particular university, are not listed. The practicum training facilities of the Veterans Administration are being evaluated and will be listed later.

The list is alphabetical by states and agencies. Following the name and address of an agency, the information about the internship and the nature of the experience it provides is given in the following order: age level of patients, types of disorders of patients, length and beginning date of appointment, amount of stipend, and any unusual additional benefits. Most of the appointments are for one year, but this includes some time for sick and vacation leave.

INDEPENDENT AGENCIES WITH APPROVED INTERNSHIPS

California

LANGLEY PORTER NEUROPSYCHIATRIC INSTITUTE, Dept. of Psychiatry, University of California School of Medicine, Parnassus and First Aves., San Francisco. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July 1 or Sept. 1; third-year USPHS stipend \$2400 for full-time, \$2094 for half-time work.

LOS ANGELES PSYCHIATRIC SERVICE, 8770 W. Whitworth Dr., Los Angeles 35. Ages 18-45; all disorders; outpatients. Appointment 1 year, beginning Sept. 15; USPHS stipend \$1200-1400 for half-time work. (Joint program with Reiss-Davis Clinic.)

METROPOLITAN STATE HOSPITAL, Norwalk. Adults; all disorders; inpatients and outpatients. Appointments: 12 months, beginning June-Sept.; stipend of USPHS fellowships plus room and board. Part-time internships allow room and board.

MOUNT ZION PSYCHIATRIC CLINIC, 2255 Post St., San Francisco 15. All ages; all disorders; outpatients. Appointment 1-2 years, beginning July or Sept.; stipend \$3000-6000.

NAPA STATE HOSPITAL, Imola. Children's Unit and adults; all disorders; inpatients. Appointment for 3-9 months, beginning at any time; no stipend.

PORTERVILLE STATE HOSPITAL, P. O. Box 2000, Porterville. All ages; mentally retarded; inpatients and outpatients. Appointment, 9 months, beginning any time; stipend \$267 per month. Apartment \$25 per month.

REISS-DAVIS CLINIC FOR CHILD GUIDANCE, 715 N. Fairfax Ave., Los Angeles 46. Ages 4-17 and parents; all disorders; outpatients. Appointment 1 year, beginning Sept. 10; stipend \$2400 for third-year, \$2800 for fourth-year student. (Joint program with Los Angeles Psychiatric Service)

SONOMA STATE HOSPITAL, Eldridge. Mentally deficient; inpatient. Neurotic nonmentally deficient outpatients. Appoint for 9 months, beginning any time; stipend, \$255 per month.

Colorado

UNIVERSITY OF COLORADO SCHOOL OF MEDICINE, 4200 E. 9th Ave., Denver. All ages, including infants; all

disorders; inpatients and outpatients. Appointment 1 year, beginning July or Sept.; USPHS predoctoral stipends; postdoctoral, \$6000 first year; \$7000 second year.

Connecticut

INSTITUTE OF LIVING, 200 Retreat Ave., Hartford 2. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July or February; stipend \$2400; free duty lunch, maintenance available at \$75 monthly.

NORWICH STATE HOSPITAL, Norwich. All ages; inpatients and outpatients. Appointment 1 year, beginning Sept. 15; stipend \$142.53 bi-weekly, tax exempt; complete maintenance available for single interns at \$316 per year.

PSYCHOLOGICAL LABORATORIES, CONNECTICUT STATE HOSPITAL, Middletown. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept.; stipend \$2400, 3000, 3720; complete maintenance free on USPHS stipend, complete maintenance at \$136 per year on hospital stipend.

District of Columbia

CHILD CENTER, CATHOLIC UNIVERSITY OF AMERICA, 4th and Michigan Aves., N.E., Washington 17. All ages, primarily children; behavior problems, psychoneurotic outpatients. Appointment 11 months, beginning Sept. 15; stipend \$1650.

ST. ELIZABETHS HOSPITAL, Washington 20. All ages; but primarily adult (may have exchange period in university child study clinic if interested); all disorders; inpatients. Appointment 1 year, beginning July 1; stipend \$2400-2600 for predoctoral, \$3100 for postdoctoral interns; rooms for single persons at \$10 per month.

Illinois

INSTITUTE FOR JUVENILE RESEARCH, 907 S. Wolcott Ave., Chicago 12. Infancy to 18 years and parents; emotional and behavior problems; inpatients and outpatients. Appointment 1 year, beginning Sept. 1; stipend \$4020-4860.

INSTITUTE FOR PSYCHOSOMATIC AND PSYCHIATRIC RESEARCH AND TRAINING, MICHAEL REESE HOSPITAL, Chicago 16. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Oct. 1; predoctoral stipend \$1500; postdoctoral appointments; free meals for all appointments.

NEUROPSYCHIATRIC INSTITUTE, UNIVERSITY OF ILLINOIS MEDICAL SCHOOL, 912 S. Wood St., Chicago 12. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 1; stipend \$2600-3200, or residency at \$1920 plus meals and laundry.

NORTHWESTERN UNIVERSITY MEDICAL SCHOOL, 303 E. Chicago Ave., Chicago. All ages including infants; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 1; stipend \$2400.

UNIVERSITY OF CHICAGO, School of Medicine, 950 E. 59th St., Chicago 37. Adult psychiatric patients and children with emotional problems. Appointment 1 or 2 years, beginning July or October. Stipend \$2400-3000.

Indiana

CHILD GUIDANCE CLINIC OF MARION COUNTY, INC., 1949 East 11th St., Indianapolis. Children through 16 years; emotional disturbances; outpatients. Appointment eleven months, beginning June to Sept.; stipend \$3750.

INDIANA UNIVERSITY MEDICAL CENTER, 1100 W. Michigan St., Indianapolis 7. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 1; stipend \$2400-4000.

LARUE D. CARTER MEMORIAL HOSPITAL, 1315 W. 10th St., Indianapolis 7. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July 1 and Sept. 1; stipend \$4620 for third year, \$5100 for fourth year students; full maintenance for single interns at \$25 per month.

Iowa

DES MOINES CHILD GUIDANCE CENTER, 500 Garver Bldg., Des Moines 9. Children; all disorders; outpatients and day hospital. Appointment 1 year, beginning June or Sept.; stipend \$3000.

DIVISION OF PSYCHOLOGY, DEPARTMENT OF PSYCHIATRY, STATE UNIVERSITY OF IOWA, Iowa City. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July 1; stipend \$3000 for third-year.

Kansas

TOPEKA STATE HOSPITAL, Topeka. Adolescents and adults; all disorders; inpatients and outpatients. Appointment 1 year, beginning July 1 or Sept. 1; stipend \$3000-4200; room and board at nominal cost.

WICHITA GUIDANCE CENTER, 3422 E. Douglas, Wichita 8. Ages 3-17, parents and other adults; behavior problems of children, neurotic adults; outpatients. Appointment 1 year, beginning July 1 or Sept. 1; stipend \$3000.

Kentucky

DEPARTMENT OF PSYCHIATRY AND MENTAL HEALTH, UNIVERSITY OF LOUISVILLE SCHOOL OF MEDICINE, 206 E. Chestnut St., Louisville 2. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning mid-year or summer; stipend \$2800.

Louisiana

PSYCHOLOGY UNIT, DEPARTMENT OF PSYCHIATRY AND NEUROLOGY, SCHOOL OF MEDICINE, LOUISIANA STATE UNIVERSITY, New Orleans 12. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July or Oct. 1; stipend \$2000.

SOUTHEAST LOUISIANA HOSPITAL, Mandeville. Children, adolescents and adults; neurotic and psychotic; inpatients. Appointment 1 year, beginning Sept. 1; stipend \$2800-4500, first \$300 per month tax free.

Maryland

PSYCHIATRIC INSTITUTE, UNIVERSITY OF MARYLAND, Baltimore 1. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July 1, some flexibility; stipend \$4500 for third year student.

SPRINGFIELD STATE HOSPITAL, Sykesville. Adults and possible rotation through institutions for children; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept.; stipend \$3060 tax free, maintenance at cost. Postdoctoral applications considered.

Massachusetts

DEPARTMENT OF PSYCHIATRY, CHILDREN'S HOSPITAL, 300 Longwood Ave., Boston 15. Ages birth to 21, some parents; neurotic, psychosomatic, and some psychotic; inpatients and outpatients. Appointment 1 year, beginning Sept. 1; stipend of USPHS fellowship, amount based on level of training.

JUDGE BAKER GUIDANCE CENTER, 295 Longwood Ave., Boston 15. Ages 5-17; neurotic and behavior disorders; outpatient treatment and treatment in residence and school. Appointment 1 year, beginning Sept. 1; stipend as granted by USPHS.

SPRINGFIELD CHILD GUIDANCE CLINIC, INC., 759 Chestnut St., Springfield; auspices of Western Massachusetts Training Program, Division of Mental Hygiene. Children, ages 2-16, and their families; outpatient. Appointment 3-12 months, beginning any time; stipend \$200-240 per month.

WORCESTER STATE HOSPITAL, Worcester. Adults; all disorders; inpatients and outpatients. Appointment 1 year, beginning July 1; stipends of USPHS predoctoral and postdoctoral fellowships.

WORCESTER YOUTH GUIDANCE CENTER, 2 State St., Worcester. Children and parents; emotionally disturbed; outpatients. Appointment 1 year, beginning Sept. 1; USPHS predoctoral stipends.

Michigan

CHILDREN'S CENTER OF METROPOLITAN DETROIT, 5475 Woodward Ave., Detroit 2. Children through high school and parents; neurotic and behavior dis-

orders; outpatients. Appointment 1 year, beginning July 1; stipend \$2360.

LAFAYETTE CLINIC, 951 E. Lafayette St., Detroit 7. All ages; emotionally disturbed; inpatients and outpatients. Appointment 1 year, beginning July 1; stipend \$3432.

Minnesota

AMHERST H. WILDER CHILD GUIDANCE CLINIC, 670 Marshall Ave., St. Paul 4. Ages 3-17; all disorders; outpatients. Appointment 1 year, beginning Oct. 1; stipend as granted by USPHS.

Missouri

COMMUNITY CHILD GUIDANCE CLINIC OF WASHINGTON UNIVERSITY, DIVISION OF CHILD PSYCHIATRY, SCHOOL OF MEDICINE, 369 N. Taylor Ave., St. Louis. Children; all disorders; outpatients. Appointment 1 year, beginning Sept. 1; \$2400-2800.

MEDICAL PSYCHOLOGY, DEPARTMENT OF PSYCHIATRY AND NEUROLOGY, WASHINGTON UNIVERSITY SCHOOL OF MEDICINE, St. Louis 10. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July-Sept.; stipend \$2400 minimum.

ST. LOUIS STATE HOSPITAL, 5400 Arsenal St., St. Louis 9. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 15; stipend \$3600; temporary living quarters.

Nebraska

NEBRASKA PSYCHIATRIC INSTITUTE, 602 South 44th Ave., Omaha. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July 1; stipend \$2400-3000.

NORFOLK STATE HOSPITAL and OUT PATIENT CLINIC, Box 902, Norfolk. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 1; stipend \$3000.

New Jersey

NEW JERSEY STATE DEPARTMENT OF INSTITUTIONS AND AGENCIES, Office of the Chief Psychologist, State Office Building, 135 W. Hanover St., Trenton 25; Child Guidance Center of Mercer County, Trenton; Trenton State Hospital, Trenton; New Jersey Neuropsychiatric Institute, Princeton; New Jersey State Hospital at Marlboro; Diagnostic Center, Menlo Park; Edward R. Johnstone Training and Research Center, Bordentown (this institution is approved only for a second year of internship supplementary to a traineeship with one of the preceding training centers in this system). All ages, all disorders, inpatients and outpatients. Appointment for 1 year; stipend \$3063 (tax-free).

New York

BELLEVUE PSYCHIATRIC HOSPITAL, 30th St. and 1st Ave., New York 16. All ages, including infants; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 15; stipend \$2510, and free lunches.

BRONX MUNICIPAL HOSPITAL CENTER and the ALBERT EINSTEIN COLLEGE OF MEDICINE OF YESHIVA UNIVERSITY, Jacobi Hospital, Dept. of Psychiatry, Eastchester Road and Morris Park Ave., Bronx 61. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 15; stipend \$2510.

COLUMBIA-PRESBYTERIAN MEDICAL CENTER and NEW YORK STATE PSYCHIATRIC INSTITUTE, 722 W. 168th St., New York 32. All ages, all disorders, inpatients and outpatients. Appointment for 1 year, beginning Sept. 1; stipend \$2400.

DEPARTMENT OF PHYSICAL MEDICINE AND REHABILITATION, NEW YORK UNIVERSITY, BELLEVUE MEDICAL CENTER, 400 E. 34th St., New York. All ages; physically disabled; inpatients and outpatients. Appointment 1 year, beginning Sept.; stipend \$2400 for third-year, \$2800 for fourth-year student.

INSTITUTE FOR THE CRIPPLED AND DISABLED, 400 First Ave., New York 10. Adults emotionally and physically handicapped; outpatients. Appointment 1 year, beginning Sept. 1; stipend \$3600-4200.

NEW YORK HOSPITAL, WESTCHESTER DIVISION, 121 Westchester Ave., White Plains. All ages; functional disorders, some neurological; inpatients and outpatients. Appointment 1 year, beginning Sept.; stipend \$3600 free duty lunch.

PSYCHIATRIC DIVISION, KINGS COUNTY HOSPITAL CENTER, 606 Winthrop St., Brooklyn 3. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 16; stipend \$2510.

ST. VINCENT'S HOSPITAL OF THE CITY OF NEW YORK, 11th and 7th Ave., New York. All ages; all disorders; inpatients and outpatients. Appointment for 1 year, beginning Sept.; stipend \$2400.

North Carolina

DUKE UNIVERSITY MEDICAL CENTER and the DURHAM CHILD GUIDANCE CLINIC, Durham. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 1; stipend \$2400, or USPHS third or fourth year stipends.

NORTH CAROLINA MEMORIAL HOSPITAL, University of North Carolina, Chapel Hill. All ages, all disorders, inpatients and outpatients. Appointment for 1 year, beginning June or Sept.; stipend \$2400.

North Dakota

STATE HOSPITAL, Jamestown. All ages; all disorders; inpatients and outpatients. Appointment 1 to

2 years, beginning any time; stipend \$2400 plus free complete maintenance for third year student, \$5880 for 4th year student, first \$300 per month tax free, also USPHS predoctoral stipends.

Ohio

COLUMBUS PSYCHIATRIC CLINIC, 1960 W. Broad St., Columbia 15. Adults; psychoses, psychoneuroses, personality disorders; outpatients. Appointment 1 year, beginning any time; stipend \$3300 for third-year, \$3600 for fourth-year student, tax exempt.

COLUMBUS PSYCHIATRIC INSTITUTE AND HOSPITAL, Ohio State University Health Center, Columbus 10. Ages 16 and up; all disorders; inpatients and outpatients. Appointment 1 year, beginning anytime; stipend \$3300-3600.

UNIVERSITY HOSPITALS OF CLEVELAND, 2065 Adelbert Rd., Cleveland 6. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept.; stipend \$2400-3000; opportunity to teach in Dept. of Psychiatry, Western Reserve University.

Oregon

COMMUNITY CHILD GUIDANCE CLINIC, 922 S.W. 17th Ave., Portland; and the Boys and Girls Aid Society, 2301 N.W. Glisan St., Portland. Adoption and behavior problems of children; Outpatient. Appointment 1 year, beginning July; stipend \$2400-3000.

DIVISION OF MEDICAL PSYCHOLOGY, UNIVERSITY OF OREGON MEDICAL SCHOOL, Portland 1. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July 1 or Sept. 1; stipend \$2400 predoctoral, \$6000 and \$7000 postdoctoral.

Pennsylvania

CHILD STUDY CENTER OF PHILADELPHIA, 110 N. 48th St., Philadelphia. Ages 2½-18 and parents; all emotional disorders; outpatients. Appointment 1 year, beginning Sept. 1; stipend \$2800.

DEVEREUX FOUNDATION, Devon. Ages 5-20 and parents; emphasis on emotionally disturbed or intellectually retarded; inpatients. Appointment 9-12 months, beginning July 1 or Sept. 1; combined Devereux and USPHS stipends \$3000-6000, first \$300 per month tax exempt; room and board for single interns.

PITTSBURGH CHILD GUIDANCE CENTER, 201 De Soto St., Pittsburgh 13. Children and parents; emotionally disturbed; outpatients. Appointment 1 year, beginning Sept. 1; stipend dependent on experience.

Rhode Island

EMMA PENDLETON BRADLEY HOME, 1011 Veterans Memorial Parkway, Riverside 15. Children; all disorders; inpatients. Appointment 1 year, beginning

Sept. 1; stipend \$2800, tax free; full maintenance for single interns at \$55 per month.

Tennessee

GAILOR PSYCHIATRIC HOSPITAL, SCHOOL OF MEDICINE, UNIVERSITY OF TENNESSEE, 42 N. Dunlap St., Memphis. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning any time; stipend \$2640.

Texas

DEPARTMENT OF PSYCHIATRY, BAYLOR UNIVERSITY COLLEGE OF MEDICINE and HOUSTON STATE PSYCHIATRIC INSTITUTE, TEXAS MEDICAL CENTER, Houston. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 1; stipend \$2400-3000; meals at hospital.

DEPARTMENT OF PSYCHIATRY, UNIVERSITY OF TEXAS SOUTHWESTERN MEDICAL SCHOOL, 5323 Harry Hines Blvd., Dallas. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning Sept. 1; stipend begins at \$2400.

PSYCHOLOGY DEPARTMENT, UNIVERSITY OF TEXAS MEDICAL BRANCH, Galveston. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July or Sept.; stipend \$2640-2880; single maintenance \$60 per month.

Utah

DIVISION OF PSYCOLOGY, DEPARTMENT OF PSYCHIATRY, MEDICAL COLLEGE, UNIVERSITY OF UTAH, 156 Westminister Ave., Salt Lake City. All ages; all disorders; inpatients and outpatients. Appointment 1 year, beginning July 1; stipend as granted by USPHS.

Approved Internships in the Department of the Army, Office of the Surgeon General

LETTERMAN ARMY HOSPITAL, San Francisco, California, and WALTER REED MEDICAL CENTER, Washington, D. C. All ages; all disorders; inpatients and outpatients. Commission as second lieutenant for internship in third year and for fourth year of graduate study, and first lieutenant on receiving PhD. Apply to Clinical Psychology Consultant, Office of the Surgeon General, Dept. of the Army, Washington 25, D. C.

NEW MEMBERS AND ASSOCIATES OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

THE Board of Directors announces that the following 1,133 persons were elected to membership in the American Psychological Association as of *January 1, 1960*. Of these, 391 were elected as Members, and 742 were elected as Associates. Though not all of them have validated their election by payment of dues, all but a few will eventually do so. In accordance with Article II, Section 8, of the By-Laws, 70 Associate members, having obtained their doctoral degrees in psychology have been transferred to Member status as of *January 1, 1960*.

MEMBERS

Abarbanel, Albert Brandt	Cloer, Harold Angus	Farley, Nancy Bess	Gormezano, Isidore
Altman, Joseph	Cohen, Haskel	Farr, S. David	Gottesman, Leonard Edmond
Anderson, Wayne Jeremy	Cohn, Benjamin	Fast, Irene	Granda, Allen Manuel
Angermeier, Wilhelm Franz	Collier, Juanita	Fauset, Charles Edwin	Green, Arthur M.
Asdourian, David	Cook, David Reed	Feierabend, Rosalind Lorraine	Green, David Marvin
Ayman, Iraj	Cooper, Arnold Melvin	Feiner, Siegfried	Greene, Donovan Riley
	Cornwell, Anne Christake	Feldhusen, John Frederick	Gustafson, Herbert Wayne
	Coulter, Walter McLean	Feldman, Samuel Mitchell	Gutmann, David Leo
	Cox, Richard H.	Filson, Thomas Newel	
	Crager, Richard Lynn	Fiske, Claude Ernest, Jr.	Haer, John L.
	Crausman, Burt	Fitzgerald, Laurine Elisabeth	Hagen, C. Larry
	Cravens, James Montgomery	Fitzpatrick, Garland Montgomery	Hagen, Douglas H.
	Crowder, William F.	Flapan, Mark	Halas, Edward Steven
	Culbertson, Ellen	Fletcher, Richard Mumma	Halfond, Murray Michael
	Cusack, Bruce Lee	Forehand, Garlie Albert, Jr.	Harris, Douglas Hershel
		Forrin, Bert	Harris, James Maurice
Baer, Jean Hitchcock	Dardano, Joseph Francis	Foster, Susan G.	Hartman, Frank Rittenhouse
Banaka, William Harold	Davis, Robert Benjamin	Fowler, Harry, Jr.	Harvey, John Adriance
Banks, James Huber, Jr.	Dayan, Soleyman	Fowler, William Louis	Harwood, Kenneth Albert
Barclay, James Ralph	Dean, Lois Remmers	Foy, Glenn Arthur	Haskell, Martin Roy
Barkman, Paul Friesen	Dean, Waid H.	Fraknoi, Julia	Heermann, Emil Francis
Barnett, Charles D.	De Young, Quintin Richard	Franklin, Ruby Holden	Heinstein, Martin Israel
Barnlund, Dean C.	Dickinson, Rita Mitton	Frankmann, Judith Parker	Henderson, Joseph Ralston
Barr, John Alton	Dill, William Rankin	Fretwell, Loretta Naoma	Herlocker, Keith E.
Beckett, Thomas	Dinkmeyer, Don C.	Fried, Marc Allen	Herod, Thomas Mack
Belfort, Anne Dorothy	Donahoe, John William		Hetherington, Eileen Mavis
Binner, Paul Raymond	Dondero, Brother E. Austin		Hickson, Robert Hugh
Bleek, Julian K.	Douglass, Donald Delbert		Hill, Walter Raymond
Bloomer, Richard Hutcheson	Drake, Henry L.	Gaither, James Wallace	Hoffberg, Caroline Meixner
Borriello, John Francis	Drewes, Donald William	Gale, Raymond Floyd	Hofmann, Helmut Philipp
Boss, Ruth Lea	Duetsch, Joseph John	Gallen, Albert A.	Hokanson, Jack Eric
Braaten, Leif Johan	Dunsing, Jack Donald	Georgopoulos, Basil Spyros	Hopkins, Robert Floyd
Bresee, Clyde Wesley	Dushkind, Donald Stanford	Gerjuoy, Irma Rossman	Horowitz, Frances Degen
Brittain, Clay Vester	Dzendolet, Ernest	Gewirtz, Herbert	Hungate, Vernon E.
Brockopp, Gene William		Giebkink, John William	Hurwitz, Irving
Brush, Helen N.		Giedt, Helen Katherine	Husek, Ted
Burstein, Alvin George	Eddowes, Edward Everett	Gleason, Jean Berko	
Butler, Donald Charles	Eddy, Raymond Taylor	Glickman, Stephen E.	Ibeling, Frederick William
	Edwards, Allen Jack	Gnagey, William Joel	Inglis, James
Caldwell, Edward	Eikaas, Alf Ingvald	Gocka, Edward F.	Ivey, Allen Eugene
Campos, Nilton	Emm, Sister Mary Eloise	Goodman, Barbara Ann	
Cannon, L. Dennis	Englander, Meryl E.	Gordon, Edmund Wyatt	Jacobs, Berne Lee, Jr.
Cannon, William John	Erickson, Robert Porter		Jacobs, Irving Aaron
Carbonell de Grompone, Maria A.			
Carino, Oliva Palafox			
Carter, Lamore Joseph			
Casas, Eduardo Fernando			
Check, John Felix			
Chenault, Joann			
Clark, Rudolph Ernest			
Clark, Philip Irving			

Jardon, Cesar Alberto
Jarrard, Leonard Everett
Jensen, Glen Donald
Johnson, Donald Everett
Johnson, Richard Morton
Johnson, Ronald Charles
Jones, A. Rayburn
Jurjevich, Ratibor Momchila

Karas, George G.
Karsh, Eileen B.
Kasper, Sidney
Kastenbaum, Robert Jay
Katchmar, Leon Tarance
Katz, Myer
Keesey, Ulker Tulunay
Kendall, John Seedoff
Kennedy, Sister Paulina Mary

Keppers, George Leonard
Keswick, Gordon Merle
Khan, Lillian
Kimmel, James
Kimura, Douglas
Kinney, Jo Ann Smith
Kirk, Brother Daniel
Kirtner, William L.
Knox, Wilma Jones
Koening, Frances Green
Koh, Soon Duk
Koppelaar, Richard John
Korot, Leonard
Krause, Merton S.
Kronvall, Ernest Lawrence

Lackner, Frank M.
La Driere, M. LaVerne
Lampman, Henry P.
Lancaster, Cornelia
Lane, Paul Anthony
LaPine, Harry John
Laurence, Mary Wright
Leblanc, A. Gaston
Leshin, George J.
Lester, James Treloar, Jr.
Levin, Saul M.
Levy, Joshua
Lewis, Madge Katharine
Lifton, Harvey
Littig, Lawrence William
Lodahl, Thomas Martin
Lodato, Francis Joseph
Lott, Albert J.
Loughary, John W.
Lucito, Leonard J.
Luttrell, Jack Shiefler
Lynch, Charles Henry

Macbride, Philip Douglas
MacDonnell, Malcolm Forbes
Macek, Albert Joseph
MacQueen, James B.
Mahone, Charles Henry
Mandel, Jacob
Mangus, Arthur Raymond
Mann, David
Marge, Michael
Marks, Alvin
Markwell, Noel Gene
Marshall, M. Jack
Martin, Joseph Robert
Mason, Geoffrey Pliny
Mathae, David Eugene
Mathews, Quinten Snow
Mattsson, Patrik Owe
Mawardi, Betty Hosmer
Mayo, Clara A. W.
McDermid, Charles Douglas
McGreevey, James Carroll
McKelligott, John William
Metz, John Richard
Miller, Carrie Eulah
Miller, Lyle Leslie
Miller, Robert Wilbur
Mohandessi, Khossrow
Mok, Paul P.
Moles, Edgar A.
Molnar, Andrew Raymond
Moore, Gilbert Donald
Morgan, Robert Marion
Morrison, Gordon Rolfe
Moskowitz, Herbert Alfred
Moss, Howard Alan
Moulton, Robert W.
Muehl, Siegmar
Murphy, Robert Emmett

Natchez, Gladys Worms
Nelson, LaVerne Wendell
Newbrough, John Robert
Ni, Katherine Kao
Nish, William Walter
Noble, Frank Channing
Noble, Jeanne Laveta
Nolan, Robert Dale
Nolte, John Moore
Nothman, Siegfried H.
Novack, Harry Samuci
Nyles, Robert David

Ordy, Joseph Mark
Orne, Martin Theodore
Orton, Kenneth D.
Owen, Wayne Albert

Paivio, Allan
Palmer, George Joseph, Jr.

Paré, William Paul
Pauk, Walter Joseph
Peckham, Robert Hamilton
Pepler, Richard Douglas
Perlman, Marcel Isaac
Pertschonok, Daniel
Peskin, Harvey
Peterson, Frederika Banning
Piety, Kenneth Ralph
Pinkard, Calvin Moore, Jr.
Posinsky, Sollie Henry
Price, Robert Preston, II
Pryer, Ronald Stanley
Psomas, Themis Tocles
Purdy, Malcolm M.

Quigley, Stephen P.

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Rahner, Brother D. William
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Comment

The Psychologist as Educator

Psychologists whose primary function is teaching are yearly growing more involved in a basic dilemma. Should our primary concern be for the development of the personalities of our students, or should it be for the development of a body of information? We frequently have the feeling that the psychologist ought to be more concerned with personality development than with indoctrination.

Consequently, our major problem is that of specific course structure and content. Should General Psychology, for instance, be taught in such a way as to develop a specific body of information, or should it be taught to help in the growth of those taking it. In the final analysis we have all probably tried to do both. Some of us, however, have been more concerned with the mastery of a specific body of information than with personal development, while others have been more concerned with personal development than with indoctrination. It becomes particularly embarrassing when two of us with markedly different objectives successively teach the same course. The problem is, of course, resolved for us when our classes have such large enrollments we cannot do anything other than use objective tests to measure content absorbed. Content is *all* we can hope for in such circumstances.

But even when enrollment is such that we can come to know pupils individually, we cannot teach purely from the standpoint of personal development. There is always the matter of grades. How does one assign grades in a course in which the emphasis is upon personal development?

Furthermore, the very fact that grades are to be given materially affects the relationship between teacher and student. Both have to keep in mind the accounting at the end of the session. How can the relationship be oriented toward personality development when both have to keep one eye on the record books?

Perhaps the answer is to be found in separating personality development functions from the teaching situation. Let the psychologist as teacher teach his subject matter and forget the personality of his student, use other psychologists in a counseling capacity to do what they can to assist the individual to optimum development of his personality.

The catch to this proposal is that so often the student is aroused to further self-exploration through the subject matter of the course or by some casual observation made in a lecture. In response to a classroom question this writer, for instance, recently commented upon the part role-concepts had in marital adjustments.

After class a graduate student asked for a conference and revealed an impending separation from his wife. The problem was pressing and immediate. I was unable to give an appointment for another five days. By the time the conference could take place the wife had left. Perhaps a counseling service could have handled the situation immediately. But more probably the student would never have approached the counseling service directly.

Actually, the dilemma in which we find ourselves is not peculiar to psychology, although it is more acute for the psychologist than for any other teacher since his subject matter impinges so much more directly upon personality than does the subject matter of other disciplines. Our dilemma is the dilemma of modern education and probably cannot be solved by us alone.

Most of society's requirements are in terms of course credits and grade points. Many credentials and licenses (witness teachers credentials) are given purely on this basis. Even when the state gives examinations for the granting of licenses, these examinations are made on the basis of course content of a recommended curriculum. Examinations are on subject matter rather than on personality and character. How can the educator do otherwise than concentrate on content and course credits under these circumstances? In other words, even the educational system as a whole is not free to make the choice. Progressive educators have long been emphasizing the necessity of teaching pupils rather than subjects, but inevitably this runs counter to society's demands.

But, the question is inevitable: what happens to professional training? Will not society suffer by lack of standardization in professional training? What about the resistance we often find in even graduate students in psychology to courses in statistics, experimental methods, etc.? The wholesome, growing personality will actually seek the disciplines which will enhance his professional qualities—if these disciplines can be shown to be thus desirable. If the requirement cannot be justified in the eyes of the student, it might well be seriously questioned by those who are recommending it. In other words, there is a reasonable assumption that a well adjusted individual training for a professional role will seek to master the skills and information which the course-credit system compels him to "take." And there is always the matter of examinations. One can hold up those with inadequate technical preparation at this point (witness state medical board examinations with their emphasis upon anatomical and pharmaceutical knowledge). Schools can

accomplish the same thing (as some are) by final comprehensive examinations for the degree.

This is, of course, the ideal. What is the immediate program? We must recognize, first of all, that the ideal is far off and demands some societal changes and changes in our educational structure. Perhaps we can aid this change by cooperating with those educational leaders who are seeking the same objective. In the meantime we can do all we can *within the structure* to help individuals in their personal development even while we have to give attention to content and grades. Can we, like progressive educators, teach students rather than subjects?

W. EDGAR GREGORY
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Convention Handouts

I have before me a sampling of 18 handouts gleaned from attendance at the Cincinnati meeting. With a view to preventing handouts from becoming throwaways, may I suggest a few standards for labeling? While a few of my samples were adequately documented, many were incomprehensible after departure from the meeting room. For those of us who feel conscience-bound to share the meetings with nonattending colleagues, this is distressing.

Minimally, such duplicated materials should contain names of authors, title of paper, and date. (Only 5 of my samples did so.) Additionally, the data presented should be labeled in sufficient detail so that such unfamiliar abbreviations as AVA or V_3 can be explained some weeks later. Arrangement of items should also be considered carefully so that listeners may follow efficiently the trend of thought of the speaker. It seems unnecessary to add that the composition of the *N* be indicated.

MARIAN R. BALLIN
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Colleagues and Criticism

One of the most pressing needs in psychology today is for serious criticism of ideas and research. Papers circulated to colleagues are often returned untouched or with the notation: "I find this interesting and well written." It is difficult to get others to consider a paper seriously. Perhaps this is one reason behind the torrent of papers submitted to journals. A writer hopes that, if he publishes an article, he may receive some useful comments. Unfortunately this happens only rarely since most articles are lost in the endless stream of publications. Using the journal editors to evaluate ideas and research is also a serious waste of editorial time. It means that an overworked and unpaid editor must make minor grammatical changes and

clarifications that should have been made on the second draft.

From the standpoint of the writer, there are a number of reasons why people are reluctant to comment upon a paper. The reader may not be interested in the paper or feel that he is not competent to evaluate it. The reader may not be sufficiently articulate to express his opinions or believe that the paper is so poor that he is unable to say anything favorable and feels that silence is the best policy. The reader may think that the author will react poorly to any adverse criticism or ignore it entirely. Finally, the task of giving good criticism is time consuming, arduous, and often thankless. The reader may not want to make the investment of time and energy necessary for useful comments.

All of these factors combine to produce a climate in which the psychologist feels isolated and that his work is of no significance. After a few reactions of "It's interesting," the writer soon learns to cease circulating his paper. Although he may feel that letting others read his paper is not exactly casting pearls before swine, he may conclude that it is like showing pictures to the blind or playing symphonies to the deaf. The scientist should not depend on the applause of the market place for inner satisfaction. This should come from a job well done and the intrinsic interest of his research. However, he is dependent upon others to learn the implications of his work and the avenues yet to be explored. The more involved a scientist is in his research, the less able he is to subject it to adequate appraisal.

Some time ago I felt strongly about destructive and negative criticism. I remember being crestfallen when a colleague savagely attacked a paper. Now, several years later, I have come to appreciate him. Contrasted with a half-dozen good friends who merely tell me that my papers are "interesting," this critic was extremely helpful. I should like to find such a person now, but I know of no one to step into his shoes. Perhaps the day of the organization-man psychologist has arrived. Everyone seems interested in adjustment, communication, and group dynamics. A gentleman's agreement prevails about criticizing someone else's paper. The practical ethic of today is: "What do I gain by making an enemy?" In my case, anyway, a good critic would not be making an enemy, but would be giving me something that I need badly and do not get anywhere else.

Several writers have adopted one or another unconventional means for obtaining critical comments, but these methods have not been very successful. One solution is for the writer to play devil's advocate and goad the audience to such a point where they cannot help but react. Instead of using the scientific subjunctive and speaking in terms of "trends," "ap-

parent relationships," and "seeming consistencies," the writer uses first person pronouns and speaks of his results as genuine. However, the disadvantage of playing the devil's advocate is that the audience will react emotionally rather than rationally. The serious consideration desired by the author may be lost.

One antidote for audience apathy is clarification of the role of the reader. The writer must inform his colleagues whether a paper is being circulated simply as information (for example, sending a published paper to a colleague as a courtesy); for evaluation (for example, sending a paper to a potential employer or editor for evaluation); or for criticism. Serious misunderstandings can result when a paper is sent for one purpose and is interpreted as being sent for another.

Looking about me, I see no place that could be called a forum for serious ideas. Professional meetings have turned into frantic multiring circuses and extended parties. Evening discussions are filled with reminiscences and minor gossip about job openings. It is almost impossible to get serious criticism of a paper read at a convention. The audience is given five or ten minutes for questions and comments. Many psychologists become bored with "shoptalk" at conventions and conferences and look upon such meetings as opportunities to escape from their wives. I am not advocating boring people on their holidays, but rather that psychologists should be given sufficient time for sport and recreation during the year so that they will be able to use conferences for serious thought.

As the Board of Scientific Affairs recently pointed out (*Amer. Psychologist*, 1959, 14, 267-271), the use of mimeographed newsletters for areas of research may be another solution. Unfortunately these tend to develop into printed newsletters and finally into new journals. Although this bespeaks a greater scientific respectability for the area of study, the functions of conveying news informally, economically, and quickly are largely vitiated. It may be worthwhile for APA to stimulate small groups of psychologists to begin such newsletters. The *American Psychologist* could catalyze the venture by periodically listing topics along with the address of the newsletter editor. The task of the editor would be simplified enormously if each contributor submitted mimeographed copies of his work and the editor's job was simply to staple together a group of papers and distribute them to interested parties.

This would return responsibility for editing papers back to the author and remove what many writers consider to be the strait jacket of standard style. Not only might a personal flavor return to scientific writing, but a few intrepid souls might inject a touch of humor into their papers. At present the use of humor in psychological writing is the prerogative of a

handful of editors and young Turks. Nine out of ten articles in APA journals would receive failing ratings on the Flesch indices of readership appeal. This may be another reason for the dearth of good, solid criticism. It is extraordinarily difficult to criticize ideas and findings written in the subjunctive. Most such phrases are semantic monstrosities. What does it mean to say: "There may be a relationship between"? Does this preclude there *not* being a relationship, and does a relationship per se have any practical or theoretical importance? Things can be related in innumerable ways: size, shape, color, cost, gender, distance, etc. Perhaps if writers said what they meant, critics would have better targets to shoot at. Karl Popper has pointed out that, unless an hypothesis is capable of disproof, it is scientifically meaningless. Writers who bury their results under a mass of equivocations and subjunctives cannot expect substantial criticism except insofar as it pertains to style. An author must state clearly the implications of his results if he expects others to read his paper thoughtfully.

Although too much criticism in a young science is unhealthy, too little criticism is even worse. Controversy brings forth a clarification of ideas and a resolution of conflicting data. From a situation in which everyone writes, few people read, and even fewer think, only chaos can result.

ROBERT SOMMER
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Certification of Psychologists

Borgatta (*Amer. Psychologist*, 1959, 14, 296-297) is quite right in noting that, despite valiant efforts to involve every psychologist in the processes of formulating legislation for psychology, many psychologists are still unfamiliar with the forms of control we have adopted. Borgatta has correctly noted that psychologists cannot consult for a fee, even though the consultation may be with a firm or agency rather than with individuals. Two considerations were involved in our adoption of this policy. The first was that of public welfare. Recent cases reported to the APA ethics committee indicate that the small industrial firm may be as much at the mercy of the charlatan as is the individual layman. This, added to the difficulty of differentiating between various forms of consultation, convinced us that it was neither desirable nor practical to attempt to control the use of the title in certain kinds of consulting without controlling it in others.

While it might still be possible to exempt academic psychologists from legislative sanctions, it seems unfair to give this one category of psychologist *special* privileges unavailable to other psychologists. Furthermore it seems desirable that all psychologists have a stake in legislation, in order that the controls instituted

should represent the best efforts of the total profession.

The problem of the effect of our legislation upon sociologically trained social psychologists is one to which the APA Committee on Relations with Sociology and the American Sociological Association Committee on the Profession have devoted a good deal of time. We now have a solution acceptable to both committees: a clause exempting sociologically trained psychologists who meet the educational and ethical standards required for certification. To the best of our knowledge, no other professional group trained at the PhD level has similar claim to a "psychologist" title.

Finally, certification of the title "Certified Psychologist" has not been pursued because our experience suggests that it is not as effective a form of social control as certification of the general title. States with legislation have found that many practitioners without academic qualifications in psychology have been eager to use the title "psychologist" and reluctant to give it up. We believe that this is evidence that the title "psychologist" does have some prestige and status, a status difficult to maintain if it is commonly used by fortune tellers, palm readers, and phrenologists. APA has adopted its present policies only after states have accumulated experience with several kinds of legislation, including the permissive certification Borgatta advocates. On the basis of this experience we believe certification of the general title achieves the maximum protection of the public consonant with noninfringement upon other professional groups.

W. J. McKEACHIE
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Elementary Psychology for Eighth Graders: Four Years Later

In September 1954, I introduced a course in elementary psychology as an eighth-grade elective at Amherst Central Junior High School in Snyder, New York, a suburb of Buffalo. After one semester of the course, which met daily, I canvassed the class for reactions. Those, and the precourse reactions, were published in the April 1956 issue of *The American Psychologist* (pp. 194-196) under the title "Elementary Psychology for Eighth Graders?"

In May 1959, I devised an information questionnaire which I asked the senior high school principal to distribute to all those members of the original psychology class who were still in our school. I made no effort to contact the 8 who had moved out of our district or who had left school; 2 had moved in 1955 before the end of the course. That left 20, all seniors, and all of whom completed the questionnaire. My purpose was to discover what of the course remained with them after four years. A summary of the results follows:

1. While all agreed that the course was sufficiently valuable to warrant its extension to more students, the replies were split 50-50 as to whether or not their interest in the subject had continued. Here are some replies:

For awhile after the course, I wanted to go into some field of psychology. Now, I have decided to become a teacher, but I feel the course had something to do with my decision.

I am still interested in psychology as applied to medicine.

I would like to have taken a second course in psychology.

I have since been interested in taking psychology in college.

It started me interested in the career of a social worker by making me aware of the different types of personalities.

I am interested in further study, but not as a major.

2. When asked whether or not the course had wrought any changes in themselves: 19 claimed that changes had taken place in their attitudes and opinions, 17 in beliefs and personal relationships, while 10 affirmed that their behavior patterns had changed as a result of the course experience. I am assuming that the implied meaning in these "changes" is "improvement."

3. The unit remembered best, by most, was the one on hypnotism. When we studied that unit I brought in a medical student from the University of Buffalo who gave a graphic and dynamic illustrated lecture on the mechanisms of hypnotism and its uses in medicine. He also dramatized the possible dangers of hypnotism when amateurs experiment with it. The other units well-remembered were: psychology of advertising, superstitions, dreams, ESP, and the development of personality.

4. The answers to the request for additional comments can be summarized by these two:

(A young man) The course added a new outlook on life to my daily living. You seem to expect some of the things people do and say. It definitely aided in getting along with people. Girls, too!

(A young lady) I have kept my notes with the hope that they will be useful in college. It was the first class I ever really took notes in. Such a course would be more beneficial if it were offered again in senior high school on an advanced level.

Because of the numbers of students who elect this course, we opened two classes in 1956; and we have had two classes, averaging 30 each, ever since.

The results of this survey have been of great interest to us here at Amherst Central Junior High School. They may prove equally as interesting to the many persons and institutions who wrote inquiring about the course after the April 1956 article appeared.

JOSEPH B. PATTI
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Psychology in Action

A PSYCHOLOGIST TESTIFIES IN COURT

J. L. McCARY

University of Houston

THE material presented in this article has been taken from court trials in which a clinical psychologist testified as an expert witness. The case presented, while not an "ideal" court case, points up some of the problems which are likely to confront psychologists called to testify. The psychologist who is inexperienced in courtroom procedures will find, for example, that the attorneys are likely to be prepared for him and his testimony—to the extent of having talked with other psychologists and psychiatrists and having read books and journals on psychological testing, validation studies, inconsistencies among psychological theories and schools, etc. This case material should familiarize the psychologist with some of the usual problems encountered in testifying as an expert witness and, hopefully, aid him in coping with such problems.

Partial transcripts from three different court cases were combined into one hypothetical case.¹ Some of the testimony, such as the long examination by the attorney who called the psychologist, was excluded since this routine material does not offer points which would be of help or interest to the prospective expert witness. Approximately four hours of testimony were reduced to about twenty minutes of recording. This condensing is apparent in the direct examination, here discontinued after the first few minutes, in which the judge overruled the objection of the defense attorney. Similar deletions and condensations were made in other sections of the recording. Despite the abbreviating, an attempt has been made for continuity in the material—except, of course, in the direct examination.

The persons involved are: attorney for the plaintiff (*Plaintiff*), the psychologist (*Witness*), attorney for the defense (*Defense*), and the judge (*Judge*).

¹ A tape recording of the testimony in this article was played to the Annual Meeting of the Texas Psychological Association in Houston, Texas, on December 12, 1958. The transcript, as presented here, has been modified slightly for purposes of publication, and all names have been deleted.

DIRECT EXAMINATION

Plaintiff: What is your profession?

Witness: Clinical psychologist.

Plaintiff: What academic degrees do you hold, and where did you receive them?

Witness: I have the BS degree from [—] University, the MS degree from [—] University, and the PhD from [—] University.

Plaintiff: And your PhD degree is in clinical psychology?

Defense: Just a minute, your honor. I'm going to object to this man's testifying as an expert witness and would like to have a discussion.

Judge: The jury will be dismissed for a few minutes. The bailiff will tell you when to return. Bailiff, show the jury to the anteroom.

Defense: Your honor, I object to this man's trying to qualify as an expert witness in this case. He is *not* a physician and is not qualified to testify on the condition of this patient. The courts don't allow testimony of a chiropractor or naturopath in these cases, and a psychologist is no more qualified to testify on the mental and physical condition of the patient than would be the chiropractor or naturopath.

Plaintiff: Your honor, I don't know how [*Defense*] knows to what this witness is going to testify, but let me assure him it is only in the field of psychology. I have started out to qualify him as an expert in his field; and, if allowed to proceed, I can show his qualifications.

Defense: I know that you are going to try to prove that some emotional or brain damage has occurred to the plaintiff, and I know that a psychologist is not qualified by training or experience to do the job of a physician.

Judge: Doctor, have you ever testified in a similar case before?

Witness: Yes, sir, in state and federal cases.

Judge: I see no reason why the testimony of the doctor should not be allowed so long as it is confined to the field and bounds of psychology. I will not allow testimony of a strictly medical nature, however. Call the jury back in, Bailiff.

CROSS EXAMINATION

(Questions by the attorney for the *Defense*; answers by the *Witness*)

Question: Mr. [—], do you take care of people when they're sick? When they need medical help, do they call on you?

Answer: Not when their illness is physical. I'm a psychologist and not a physician—I thought that had been made clear earlier.

Q: Then you admit and say to the jury that you cannot judge and pass on the condition of the plaintiff.

A: I admit and say no such thing. What I said was that I am not a physician, and I now say that as a psychologist I can give information on the psychological condition of this patient.

Q: The comments you make on this case will, then, be limited to the field of psychology and not to medicine. You are not qualified to practice medicine, are you, Mr. [—]?

A: My opinions will be limited to psychology; and I am not, have not been, and have no intention of practicing medicine.

Q: I just want the jury to know that you are a psychologist and are not a licensed physician qualified to treat people with medicine. Are you a licensed psychologist?

A: Texas has no licensing or certification for psychologists.

Q: In other words, anyone can call himself a psychologist who wants to, can't he? He can put up a sign saying he is a psychologist, make himself some little inkblots, and go into business; is that right? Ready to make money.

A: Yes, but that does not make him qualified to do psychological work.

Q: But he could do just that, couldn't he—in Texas, anyway.

A: I'm sorry to say that is correct.

Q: And what would it take to make a person qualified to be a practicing psychologist?

A: We usually think of a psychologist as having a doctor's degree from some recognized university.

Q: Well, now, lots of people have doctor's degrees and aren't qualified to be a psychologist, are they? I have a doctor's degree myself, but I don't try to be a psychologist—people don't go around calling me "doctor" just because I happen to have a doctor's degree. I'm not a physician, and I don't expect to be called doctor.

A: I'll call you doctor, Dr. [—].

Q: I don't want you to call me "doctor."

A: Well, you don't refer to me as Dr. [—], but at least I will respect *your* title, doctor.

Q: Let's get on to your test findings.

A: All right.

Q: Mr. [—], the Wechsler-Bellevue test is a common, well known intelligence test that has been used for many years, is that correct?

A: Yes, sir.

Q: It is a test they give people. Lots of companies give them the same tests when they employ personnel, don't they; and they frequently give this test to children in schools?

A: Well, there are different forms of the test for children.

Q: It is basically an intelligence test, is it not?

A: Yes, sir.

Q: It starts off, as I recall, the first series of questions have to do with general orientation.

A: Yes, sir.

Q: The first test, if I'm not mistaken, the first question you ask—maybe they have changed it since I last saw it—but it used to be: "Who is president of the United States?" Is that still the first question you ask?

A: Yes, sir, on one of the forms it is.

Q: Then you go on and get progressively harder and towards the end of the test, it has been a long time since I have seen it, but there are some questions about the Bible that you would have to be a pretty good student of the Bible, a pretty good Bible student, to give the answer?

A: If not a Bible student, certainly an intelligent person.

Q: This test, the Wechsler-Bellevue, was not standardized on a Negro population, was it? That is, when they first gave the test to see what it really meant, Negroes were not used as part of the sample, were they?

A: No, no, they weren't.

Q: Then how can you draw any conclusions on this patient—a Negro—from a test based on answers of a white population?

A: We can still get a fairly accurate picture of an individual's performance. There were lots of races, ages, and other groups who were not used in the original sample; but the test is still valuable to us.

Q: Actually, all it is, it shows the level of a person's intelligence?

A: Within limits I'd say that is correct.

Q: That, of course, would give you no information about how long a person had been in that particular stage of mentality.

A: Within rough estimates one could say more. For example, if a person gives correct answers on difficult items and yet misses on a number of easy items, one might assume that the person at one time had the ability to learn the difficult things, though for some reason or another he has now missed the easy ones.

Q: That was not true in this case, was it?

A: I believe she did get some of the more difficult items, and there was other evidence on the test that she had perhaps performed in the past at a somewhat higher level than she was performing over-all at the time of the testing.

Q: Of course, you have no idea what her intelligence test would have been if given on the twelfth day of May, 1956, do you?

A: There is no way I could tell for sure. Though I estimate, because of her particular behavior pattern and responses to test items, the present functioning must have been in existence for some little time; but just how long I could not say.

Q: I understand she has had some higher education, higher than a great many colored people have. You could tell from the fact that some of the questions that were on the difficult side that she did have some idea about them?

A: Yes, sir.

Q: You could tell from that, that she at least had been exposed to it?

A: That she had been able to learn. It takes a certain level of intellect to learn certain things.

Q: But insofar as how long or what the test would have shown if given six months before, you would have no way of knowing?

A: If you spread that out a little bit longer, I would say that is about correct. I could not say what her intellect could have been five years ago or three years ago, but I would estimate that her present functioning has been with her for at least the six months you mention.

Q: Basically, Mr. [—], what you found on the Wechsler-Bellevue test is that she isn't real brilliant so far as her intelligence?

A: What I found, Dr. [—], was that she is not now functioning at even an average level of intellect, though I estimated that her normal level of intelligence would enable her to do near college level work.

Q: You can't say how long that condition has existed?

A: No, sir.

Q: All you can say is that she at one time might have had capacity to learn but that she is not now . . .

A: Not performing at that level.

Q: Not performing too well mentally?

A: That is correct, sir.

Q: Insofar as the Rorschach test—that is the inkblot test, is that right?

A: That is correct.

Q: So the jury can understand that, you have a series of pictures that don't mean anything at all, haven't you?

A: They are inkblots.

Q: What it is, simply, you take a blob of ink, drop it on a piece of paper, then blot it up so there is something that doesn't mean a thing in the world. It is just the way that inkblob happened to hit the paper and the way the ink ran when blotted over, am I right?

A: Yes, that's fairly close.

Q: It is quite well known; and psychologists are aware that of the many, many pictures and plates Hermann Rorschach developed, none of them meant a thing in the world—he didn't make them that way, they happened to fall that way, isn't that right?

A: Yes.

Q: And now the psychologists have taken the ones suggesting some event; and they know from experience that, if you showed this plate to someone, this inkblot that doesn't mean anything, and they say: "What does that look like?" And someone will say: "That looks like a cat." Someone else will say: "That looks like a bear or like something someone goes out and shoots." And you know what sort of personality that sort of person may have, don't you?

A: Not by any means. That which you describe is more like the content, and other factors are far more important in the test. For example, the way in which they give the response and the reason they give.

Q: You psychologists squabble quite a bit over just how valuable this inkblot test is, don't you? I mean some of you say it is absolutely worthless, don't they?

A: Certainly there is disagreement as to the value of many instruments used in psychology. There are also disagreements as to the value of many drugs in medicine. That doesn't keep physicians from using the drugs to an

advantage. And so it is with psychological tests: just because there is some disagreement as to the value doesn't mean it is not a good and valuable instrument when used by certain specialists.

Q: In other words, the Rorschach test is a personality test?

A: It is a personality test—yes, sir.

Q: Now, I notice you as a psychologist in making your report, you give not only what she says it looks like but you also comment on her reactions at the time?

A: Of course, you have her protocol right there before you. She was asked questions, and other procedures were also used in order to get additional information from her.

Q: For instance, on Plate No. 2 she said: "That looks like Santa Claus boots." And then later on she says: "It looks like two bears." Then you can tell from that what sort of personality she has?

A: I certainly would not say from any two responses or even from one entire test one could tell what sort of personality she has; though by using an entire test battery made up of several tests, one could then perhaps tell quite a bit about her personality.

Q: To summarize this test then: you psychologists assert that not only by the person saying what the inkblots remind them of but also why they say it and their actions as they go along, you can diagnose their mental condition from that?

A: Yes, sir, at least that helps a great deal in diagnosing the mental condition.

Q: You would not call that an exact science, would you?

A: No, but more exact than you are implying.

Q: I mean a lot of people disagree . . .

A: Of course there is some disagreement, as in all fields—physics, chemistry, and even law, counselor.

Q: Now, don't misunderstand me. I'm not casting aspersions on the Rorschach test, but I am merely pointing out it is not a test, for example, similar to the Wassermann test or the electroencephalogram or some test like that. It is a test of the interpretation of a person's personality where the individual psychologist who makes the test can come up with an entirely different answer than some other psychologist?

A: You are rather extreme in your statements, counselor. You probably know that there are studies which show that psychological diagnostic procedures such as these are as accurate or even more accurate than a clinical examination and/or an electroencephalogram, for example, when making check of brain damage.

Q: We know generally what those two are, and you come up after your intelligence test with the proposition that this woman isn't functioning mentally as well as a good many people do?

A: That's right.

Q: And that she shows she has probably been sometime in the past exposed to some high degree of learning but not showing it very much now, and on the psychological test or the Rorschach test you determined she has schizophrenic tendencies, if that is the proper way to express it.

A: Yes, schizophrenic behavior I believe I said.

Q: What is schizophrenic?

A: That is a form of mental illness. It keeps about one-fourth of all hospital beds in this country occupied. It is a type of insanity that a person has. It is not always perceptible on the surface. These people go along, oftentimes at a fairly controlled level, but under certain pressures or conflicts they might break down and then perform in a rather bizarre uncontrolled way.

Q: What causes schizophrenia?

A: I wish I knew that.

Q: That is a question to which no one can make an honest answer, isn't it?

A: There are many theories as to the cause of schizophrenia. Present evidence indicates that many things can cause it, and I believe that it is related not only to functional factors but also to organic factors.

Q: That is the more modern trend of psychiatry, isn't it, doctor?

A: I believe that is correct. Counselor, you slipped and called me doctor.

Q: Sorry. [Smiles] Incidentally, the more modern psychiatrists don't like to use these terms we used to use in dealing with this mental illness, do they? Going back to the old-fashioned way, you used to have simple mental depression, psychosis, paranoia, five or six that you tried to fit somebody within that particular framework; and after all is said and done, we found out that just won't work. What you are dealing with is a mental illness, all you are doing when you say schizophrenia is to indicate an illness which is manifested by certain symptoms that show up in most cases, is that right?

A: That's true.

Q: Once in awhile you get one that doesn't fit the pattern, don't you?

A: I don't understand you.

Q: What I am getting at, it doesn't convey to my mind the same thing as if you would say tuberculosis, because tuberculosis is a definite set disease which is caused by a definite organism.

A: But you don't know whether TB is of the bone or chest, do you?

Q: But you could define it further.

A: So you can break schizophrenia down into subtypes.

Q: The point I am making to this jury is that schizophrenia isn't a disease like tuberculosis which has one definite cause and can and does bring on death. But schizophrenia, all it is is a mental illness which may have many causes, isn't that correct?

A: I don't understand your analogy, but I think you're right.

Q: Schizophrenia is commonly referred to as split personality, is that correct?

A: Generally correct.

Q: We have thousands and thousands and thousands of people walking the streets today who are suffering from schizophrenia in a mild form but are not occupying those hospital beds, don't we?

A: Yes, until something happens, until pressure comes to bear and they break down and do some asocial act which they would not have done if we had been able to recognize and control their illness.

Q: And sometimes they break down without any pressure?

A: Not without any pressure. There is pressure to them; otherwise, they would not break down.

Q: But what you are saying, doctor, is that you found this woman, this one in particular, mentally ill or certainly with a tendency toward a type of mental illness, the cause of which is not known?

A: I believe you have my impressions in the reports before you. I have forgotten exactly what . . .

Q: Well, schizophrenia is mentioned in both of the reports, and in the first one I believe you referred to it as schizophrenic-like behavior I think. Well, wouldn't that just about summarize it? That what you have found is a woman not of a very high level of intelligence, who is not functioning mentally as well as she might, and she has certain tendencies which, if pressed too far, might develop into a true schizophrenia?

A: I think she is a true schizophrenic now. I think her control is adequate to keep her from showing this behavior at all times; but I think, when pressure comes to bear, her control will lessen and then it will show at all times—this schizophrenic-like behavior will come through at all times, like tuberculosis which you discussed a few minutes ago. And, of course, you know physicians state that TB can be dormant—it can be in the chest but covered up momentarily but capable of being activated under certain stress conditions. And I didn't say the patient was not of a very high level of intelligence. I said she was basically above average but not functioning properly at present.

Q: How many people walking the streets today would be subject to the same description, say they are schizophrenic, and, when pressure comes to bear, will show symptoms?

A: We recognize there are as many people outside mental institutions who should be inside as there are people inside.

Q: That's the point I'm making. We have heard it raised in a little more indelicate language that most everybody is crazy just a little bit, and it just takes something to bring it out.

A: I think every person has his breaking point.

Q: In other words, we all have these inherent tendencies, call them schizophrenia or manic depression.

A: No, sir, I would not say we have *inherent* tendencies towards schizophrenia or manic depression.

Q: Well, we all have certain tendencies from norm.

A: We all have personality tendencies, but these depend on the stresses we experience, of course.

Q: Some of us will go the schizophrenia route, and some of us will go the manic-depressive route; some of us will go some other route. Is that what you mean?

A: Some go in each of these, but a lot of people do not develop schizophrenia or any other mental illness.

Q: But would if pressured?

A: Every person has a breaking point. Put a man in front line trenches for six months day after day, and he might break. Put him under everyday activity for a lifetime, and he probably wouldn't break.

Q: Why is it some people break, and some do not?

A: It might be because of any number of different reasons.

Q: Probably in the case of schizophrenia more than any other classification, it is probably something going back a long, long time.

A: That is one belief, but by no means is it universally accepted.

Q: Well, from the Freudian point of view, the causes of schizophrenia probably go back to childhood, don't they?

A: Yes, sir.

Q: Of course, we have no way of knowing what this woman's childhood might have been or what experiences she had.

A: I don't, at least.

Q: Did you get any clue to that in your Rorschach test?

A: No, sir, I didn't go into background. All the referring physician wanted was a diagnosis, and I made that from the psychological tests.

Q: You didn't see her as a patient, you didn't recommend treatment?

A: I thought she might benefit by psychiatric care.

Q: There is great dispute, of course, in modern psychiatry that these things are functional or organic, or whether or not they can be organic?

A: That's right.

Q: At any rate, the profession of medicine or psychology hasn't gotten to the point where they can put their finger on it and say what organic lesion causes it?

A: No, sir.

Q: When you examined this woman, you found she was not functioning mentally as well as the average—we will not use the word normal because I understand you psychologists do not like to use that word. Anyway, the average, and that the behavior which she exhibited under pressure indicated that the mental weakness she had would be characterized by this word schizophrenia or split personality.

A: What I said in the report—I think—was that she is now schizophrenic but is able to control these tendencies ordinarily, but under pressure will likely show the signs and that she does show a loss from her former intellectual level.

Q: And, of course, what that is due to, you cannot say?

A: No.

Q: That is all.

REDIRECT EXAMINATION

(Questions by the attorney for the *Plaintiff*; answers by the *Witness*)

Q: You did express in your opinion that she showed some evidence of an organic disease, did you not?

A: Yes, sir.

Q: What was your statement on the prognosis?

A: The prognosis, I felt, in this case was poor, particularly as I first saw her in April and saw her six months later and found no improvement whatsoever.

Q: Can you tell us the difference between functional and organic?

A: Organic implies some damage to the central nervous system, to the brain or something of that sort, some specific damage to it. By functional we mean psychological or emotional cause.

Q: And in your opinion there was organic disease?

A: I found some signs of organic damage, and the limited case history that was made available to me indicated the likelihood of organic factors being present.

Q: That's all, thank you, doctor.

RECROSS EXAMINATION

(Questions by the attorney for the *Defense*; answers by the *Witness*)

Q: Are you familiar with her previous history at all, doctor?

A: Not to any great degree. I am under the impression that she was in some sort of an accident.

Q: I was referring to the time before the accident.

A: No, sir, I'm not.

Q: From a psychological point of view would it have any bearing if you knew she had fallen and broken her arm in 1954, and in 1955 asserted a claim against another concern for the same injury that she had in 1954; would that have any bearing from a psychological point of view?

A: I would not say it would have any bearing on my findings. As I say, I don't know how long this pattern of behavior has been in existence. I don't know how the injury to her arm . . .

Q: I am talking about it from a psychological point of view: a person has an injury and at a later date blames it on another cause.

A: I certainly believe I would have to have more information than this. If I knew other patterns of behavior were exhibited during this time, I could say something about it; but from this information I don't believe I could say anything at all.

Q: Surely as a doctor you could comment upon this and give your expert opinion.

A: You are asking me now to give an opinion on hypothetical assumptions, and earlier you expended considerable energy to make sure I would not give any opinion except that based on my findings from the psychological tests. Surely you don't want to disregard your earlier argument. At least, I don't wish to do so, and I think I would have to have more information before I could offer an opinion.

Q: That is all.

PSYCHOLOGISTS IN INDUSTRY IN THE UNITED KINGDOM AND WESTERN GERMANY¹

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THE roots of American industrial psychology are deeply imbedded in the history of psychology in two European nations, the United Kingdom and Germany. Viteles (1932), in his extensive review of the early literature in industrial psychology, cites relatively few references from other countries outside the United States but many from the German and British literature. In fact, in the early development of the field of industrial psychology, both countries moved ahead of the United States (Kornhauser, 1930).

A sabbatical leave made possible a brief sojourn in these countries, primarily for the purpose of visiting industrial psychologists. Thirty-four psychologists were interviewed in the United Kingdom. In Western Germany 11 different organizations were visited, and 27 persons interviewed. Eighteen of them were psychologists; the others, persons in related fields. All were associated with industrial psychologists in their work. These visits included research institutes, consulting institutes, and psychology departments in large industrial firms. The number of industrial psychologists in each of the countries is not so great but that it was possible to learn from some of them something about the activities of most all of them.

The concern was with the activities of professional psychologists employed by industrial organizations. Certain arbitrary boundaries have been imposed, excluding applications of psychology to the field of merchandising.

NUMBER OF INDUSTRIAL PSYCHOLOGISTS

Conversations about numbers with industrial psychologists in both countries, checked by entries in the *International Directory of Psychologists* (Jacobson & Duijker, 1958), would indicate that there are approximately 100 persons in the United Kingdom who, on

the basis of research, teaching, or direct application in government or industry, might properly be called industrial psychologists. Counting only those whose principal occupation involves direct applications in industry, there are about 40. This number includes psychologists employed by industrial firms, including government owned enterprises, and by industrial consulting firms. It excludes university faculty, staff of research institutes, and psychologists employed directly by the military.

There are approximately 200 industrial psychologists in Western Germany. Virtually all of these people are actively employed in industry, private and government owned; in consulting firms or private practice performing a direct service to industry; and in research institutes-financed, for the most part, directly by industry and working on industrial problems. It would be more accurate to state that this was the count in 1958. The number continues to increase.

By the end of World War II there were almost no psychologists employed in industry anywhere in Western Germany. It was not until about 1949 or 1950 that there was any beginning of the reconstruction of industrial psychology (Bornemann, 1954). At the time of his writing Bornemann states that there were "about twenty industrial psychologists employed in the full-time service of larger industrial concerns" (p. 20). By 1958 this number had increased to about 80. Other aspects of industrial psychology seem to be showing equal growth.

In the United Kingdom there are a very few psychologists in industrial consulting firms, and the rest are approximately equally divided among four groups: those directly in the employ of industrial firms, psychologists in research institutes, members of the faculties of universities and technical colleges, and psychologists employed by the government civil service (which includes nationalized industries).

In Germany the distribution is quite different. The number of psychologists in academic positions who identify themselves with industrial psychology is extremely small, probably less than a half dozen in all of Western Germany. About 40% of industrial psychologists are employed directly by industrial firms, and the remainder are divided approximately equally among consulting firms (including individual private practice in industrial psychology), research institutes, and government. Most of those in the government

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category are involved, primarily if not entirely, in vocational guidance activities.

HISTORY

Working principally in the Industrial Health Research Board and the National Institute of Industrial Psychology, industrial psychologists in England early became active in a much broader range of problems than did their American colleagues. Actually, however, while industrial psychology in England in the years between the two world wars seems to loom large in perspective, it did not come to be a really major influence in British industry.

Unquestionably, during these years in England, C. S. Myers, the Director of NIIP, was the outstanding influence. The nature of the activities of psychologists in industry in the United Kingdom at that time seems to be reflected in the chapter headings of one of Myers' books (1920): "Movement Study," "Fatigue Study," "Selection Study," "Restriction of Output," "Systems of Payment," and "Industrial Unrest." References to the work of F. W. Taylor and the Gilbreths are plentiful. It was an industrial psychology that bore a most striking resemblance to what we in America think of as industrial engineering.

But, as Kornhauser (1930) pointed out, it was in Germany that, during this period, industrial psychology really flourished. At least in numbers of organizations employing personnel to work on the same broad range of human problems as in England, Germany seems to have been far in the lead. This work was done largely in institutes connected with schools of engineering and laboratories established directly by various industrial firms. While many professional psychologists were directly concerned with these efforts, it appears that a goodly portion of the work was done by engineers trained in psychotechnological methods. At least there are industrial psychologists in Germany today who look just a bit disdainfully upon this earlier *Psycho-Technik* as not being true industrial psychology.

Industrial psychology in Germany by 1930 was not limited to the applications of psychotechnical methods. There was emphasis on the development and use of tests of personality and character for use in personnel selection. There was participation by psychologists in industrial training programs. In addition, German industrial psychologists were studying workers' attitudes and other factors in motivation and other problems in the area of an industrial social psychology. Probably the outstanding individuals were Poppelreuter at the University of Bonn and Moede at the University of Berlin.

For a time psychology became quite strong in the military organization. But this did not last. The Army and the Air Force were largely under political control.

The political leaders were interested in securing officer commissions for politicians and others who agreed with them. The psychologists disagreed. This influenced Hitler and Goering, in 1942, to abolish all psychological units in the Army and Air Force and to transfer all psychologists to front line duties.

It was notable, however, that this order did not affect the Navy. Psychologists continued with the Navy until the end of the war. But, as one German psychologist expressed it: "The Navy always was a little on the other side." However, Fitts (1946, p. 160), in discussing various factors contributing to the termination of the German military psychology program while the war was still in progress, concludes that practical and scientific considerations as well as political factors were involved.

UNIVERSITIES AND INDUSTRIAL PSYCHOLOGY

To a very great extent, industrial psychology in both the United Kingdom and Western Germany has had to develop without the help of the universities. There seems to have been a wide gulf between the theoretical psychology of the university and the practical applications of the industrial psychologist.

In Britain, psychology, itself, is still a developing field. There was the long period of the war, during which psychology was not a field exempted from military service. This reduced the supply. Even now there are only about 200 psychology graduates a year, at the baccalaureate level, from British universities.

Another factor that seems to retard the development of psychology in industry is the lack of knowledge on the part of management of what psychology can offer. With some exceptions, the only students in Britain who have enrolled in psychology courses have been the potential psychologists. There is a tendency for British management, if they think of psychology at all, to consider it as something akin to fortune telling. This is much less the case in Germany where increasing numbers of students are attending lectures in psychology. There is change taking place in this regard in Britain. The universities are preparing an increasing number of professional psychologists for industry. In the technical colleges psychologists are being added to faculties to give attention to human problems in industry in management courses. At least one university, Edinburgh, is also offering a course in general psychology for general students, with an enrollment of about 320 in the 1957-58 academic year.

In Western Germany the industrial psychologist gets even less help from the universities than in Britain. Nor has his university training oriented him toward the practical nature of the problems he must face in his profession.

For the most part training in the field of industrial

psychology is given at some of the advanced technical colleges (*Technischen Hochschulen*). Outstanding is the work of Peter Hofstaetter of the Advanced Social Science College (*Hochschule für Sozialwissenschaften*) at Wilhelmshaven. As industrial psychologists were interviewed in Germany, attention was also repeatedly called to the work of Arthur Mayer of the Advanced Management College (*Wirtschaftshochschule*) at Mannheim.

RESEARCH INSTITUTES

Though the amount of research into problems of industrial psychology at the colleges and universities is limited, there is some. There is increasingly more in the United Kingdom than in Germany. Very active in research, much of it of value to industry, are the Medical Research Council Applied Psychology Units at University College, London and at Cambridge. While these units have connections with the respective universities they are distinctly government supported activities. Some research in problems related to industrial psychology is being sponsored by another governmental agency, the Department of Scientific and Industrial Research. This agency is in the peculiar position of having research funds available and going begging because there are not people to do the research.

The National Institute of Industrial Psychology, presently under the direction of C. B. Frisby, has long served somewhat as a balance wheel for psychological activities in industry throughout all of the United Kingdom. It serves a research function as well as a function of direct service to industry.

The Tavistock Institute in London is another organization active in research applied to industry, specifically human relations problems. The institute is an outgrowth of the Tavistock Clinic, but is now an independent organization, though occupying the same building as the clinic and with much interaction between the two staffs. In that part of the Tavistock Institute which deals with industry, principally, all members of the staff are trained in two disciplines—psychology, sociology, or social anthropology—and all have been or are going through psychoanalysis.

There are a few organizations interested in research in geriatrics including the problems of older workers, e.g., the Unit for Research on Employment of Older Workers at the University of Bristol. There are also a very few trade organizations employing a psychologist each. These include an association of shoe manufacturers and an association in the woolen industry.

In Western Germany there are many industrial psychology institutes. Most of them perform only a direct service function as consultants to industry. A few perform a research function. Among them are

such organizations as the Research Institute for Industrial Psychology and Personnel Organization (*Forschungsinstitut für Arbeitspsychologie und Personalwesen*), commonly known as FORFA, with headquarters in Braunschweig and offices in other cities; the German Association for Personnel Organization (*Deutsche Gesellschaft für Personalwesen*) in Frankfurt am Main; the German Research Institute for Aviation (*Deutsche Versuchsanstalt für Luftfahrt*) in Hamburg; and the Institute for Safety in Mining, Industry, and Traffic (*Institut für Sicherheit in Bergbau, Industrie, und Verkehr*) in Essen. This last named institute, for example, performs both a direct service function and a larger research function for the industries which support it, including research in psychological factors related to industrial and traffic safety and in the selection of drivers and operators of automatic machinery. There are many similar institutes throughout Western Germany, the ones mentioned being but examples.

Quite outstanding among research institutes of all types in Germany is the Max Planck Institute for Industrial Physiology in Dortmund. As the name implies, this is primarily an institute for *physiological* research. However, much of the work that is done by psychologists in the United States—particularly some of what we think of as engineering psychology, the integration of man and machine—is done principally by physiologists in Germany. The staff at the Max Planck institute includes several psychologists. They are at work on problems of performance capacity, climate and work, rest pauses and diurnal variability, monotony, fatigue, industrial safety, and other problems that lay the groundwork for the typical European interest in work methods. While there are more psychologists interested in work study from a mechanical standpoint in the European countries than there are in America, there is also a considerable group of European industrial psychologists who oppose this point of view. One is told repeatedly by psychologists concerned with personnel selection in German industries: "We must look at the whole man."

PRESENT-DAY ACTIVITIES

United Kingdom

The British have largely replaced the term "industrial psychology" with "occupational psychology." What was the Industrial Section of the British Psychological Society has just recently become the Occupational Section. The older term is considered too limiting, carrying too much the implication of applying only to factory work to the exclusion of professional work, work in offices, sales organizations, and in agriculture (Frisby, 1956; National Institute of Industrial Psy-

chology, 1957). Occupational psychology also includes activities in a military setting (Rodger, 1955).

The present study is concerned with industrial psychology in the more limited sense. Those who are actively employed in the application of psychological principles in industry include only about 40 people. These are not, all of them, industrial psychologists within our American reference. Those who are employed directly by industrial firms are usually in the capacity of personnel officers. Seldom in industry (or anywhere outside the schools), the English point out, is the psychologist labeled a psychologist. When he is so labeled, he may be expected to be a mind reader or may be consulted by his employers and colleagues concerning the vocational guidance problems of their children.

Rodger (1955) seems to have outlined the field of occupational psychology in Britain most adequately, as follows: problems of fitting the man to the job, including vocational guidance, personnel selection, and training; and problems of fitting the job to the man, including methods study, equipment design, and working conditions.

Vocational guidance. British industrial or occupational psychologists discuss vocational guidance a great deal. However, vocational guidance services seem to be reaching only a small percentage of those who might have need for such service.

Personnel selection. This seems to be the largest single activity of psychologists in industry. Several large British firms employ one or more psychologists (though they are not always so labeled). Their work involves the development and administration of selection programs not only within the United Kingdom but also within Britain's famed far-flung industrial empire.

When tests are used in selection, they are ordinarily intelligence tests, and tests that measure intelligence as a general factor. One is told that "the English favor the Burt view of a general factor." Validity studies are not as common as one might wish. The assumption is that such tests as are used are analogous tests. Consequently, there is little concern with criterion research. The explanation is that "Sir Cyril Burt and others have always emphasized the superiority of an internal criterion," and this seems to settle the issue. Actually the extreme labor shortage seems to preclude the necessity of the use of selection techniques, and psychologists have not convinced management that they have much to offer in insuring the most effective use of manpower.

NIIP provides a consulting service to industry on personnel selection, as well as other areas. Psychological consulting firms, as we know them in the United States, have not come into existence in Britain. There are possibly four or five psychologists either heading

or employed by management consulting firms. The services of these firms are not entirely, or in most cases even principally, psychological in nature.

Training. A few psychologists hold positions as training officers in industrial firms. Others, in their capacities as personnel officers, have responsibility for training programs. Both NIIP and the Tavistock Institute work with industries in the development of training programs. Much of this work is in connection with the extensive apprentice training found throughout the United Kingdom, as well as with other aspects of staff training.

Management development programs seem to be in their beginnings, with the involvement of some psychologists. Human relations training, now so popular in America, is barely getting started in Britain. The Tavistock Institute and one or two other organizations are giving considerable attention to problems of human relations in industry, but these are principally research efforts. The actual conducting of courses or study groups designed to improve the human relations skills of supervisors is found infrequently in Britain.

Just as there is considerable interest in research in problems of older workers, there is attention to their rehabilitation. There is also attention to problems of rehabilitation of the disabled.

Methods study. Those psychologists who are concerned principally with problems of fitting the man to the job are frequently not in agreement with their colleagues whose concerns are mostly with problems of fitting the job to the man. While methods study or work study has a long established place as an application of industrial psychology in the United Kingdom, it is questionable how much is done by psychologists, themselves. More typical, perhaps, is a program being run by NIIP: training nonpsychologists from industry in work methods. There are a few psychologists in industry who do spend a part of their time on problems in this category. There is a big emphasis in industry in work study, largely fostered in recent years by the Imperial Chemical Industries Ltd. of London.

Equipment design. The Medical Research Council Applied Psychology Unit at Cambridge is busy on problems concerned with fitting machines to man. There are other research activities of this sort. There is some psychological work on equipment design in the military. But there are, at most, five psychologists directly concerned with equipment design who are employed by industrial firms.

Working conditions. The Tavistock Institute is both doing research and serving as a consulting organization in problems related to technical change and social organization. There is activity of a research nature on problems relating to social factors in the working

environment as related to production and concerning incentive payments and other economic factors. There is research activity concerned with physical aspects of the working environment. But there seem to be few psychologists employed in industrial establishments for the purpose of doing anything about working conditions.

Western Germany

The German industrial psychologists are mostly men who have come through the advanced technical colleges and universities since the war. There are a few psychologists trained before the war who managed both to remain in Germany and to survive the war. Theirs has been the task of rebuilding something of industrial psychology and of providing some precedents for the younger men. On their shoulders has rested the responsibility of providing the continuity of development of the field.

The things that they are doing have a strong surface resemblance to industrial psychology in the United Kingdom. But there are many differences in the ways in which they do them. The activities of industrial psychologists in Western Germany involve vocational guidance, personnel selection, training, accident prevention, and productivity.

Vocational guidance. In Western Germany vocational guidance activities are considered a part of the general field of industrial psychology. However, vocational guidance is restricted by law to psychologists employed by the government to serve as examiners and vocational advisers in offices scattered throughout the country.

Personnel selection. Psychologists in German industry are overwhelmingly concerned with two major activities: personnel selection and training. Historically, the older activity is personnel selection and, with exceptions, is still predominant. When a West German psychologist states that he is involved in personnel selection activities, he means that he gives psychological tests. He gives tests to select apprentices; to select the instructors for these apprentices; and to select, from among the apprentices, young men who are to attend college or university on a part-time schedule. He gives tests to select automobile drivers and operators of some of the newer and more complex machinery. He gives tests to select foremen and managers.

The tests used are not the simple tests used by engineers in the days of the old *Psycho-Technik*. Now, only psychologists give tests. In giving tests the German psychologist is typically searching for two things: the test scores and the *ausdruck* or "expression." Instruments used include intelligence tests of the paper and pencil variety, performance tests, and various kinds of complex reaction tests. Much use is made of

projective techniques, though many psychologists reject the Rorschach. The one procedure used most frequently is handwriting analysis.

German psychologists are interested in characterology, in a diagnosis of the total personality. Many feel that test scores do not give as much information as does observation of the man while he takes the tests. There is more emphasis on a clinical interpretation of test results than on a statistical interpretation.

There is much stress on face validity. In order to secure the cooperation of the people being tested, the test given must appear to be measuring the "profession" or occupation for which it is being used. Then there is the necessity of a logical validity. This was best expressed by one psychologist in much these words:

It is a German quality to accept tests only if they can give rational reasons for them. To show that they are effective is not sufficient. It is a cultural trait. Normally you don't find Germans applying things that they don't understand.

There is, however, an increasing emphasis on statistical methods and empirical validity.

Skill training. Every company in Western Germany commonly has a certain number of apprentices in proportion to the number of workers employed. In a number of industries psychologists are in charge of the apprentice training programs. One company was visited where psychologists were directing the training of 400 handwork apprentices and 1,000 chemical technicians. Psychologists are involved in various other aspects of employee training as well.

Supervisory and management training. Some German industrial psychologists now consider their most important task to be the teaching of leadership methods. Sometimes this seems to be an attempt to imitate American-type supervisory and management training programs. In other situations there seems to be an implicit trust in the value of knowledge. Basic principles of psychology are taught to supervisory personnel on the assumption that knowledge of these principles will result in improved leadership behavior. Quite frequently psychologists direct programs of supervisory training for foremen and middle management personnel. Only a very few companies attempt any genuine supervisory training at higher levels.

To the extent that supervisory training programs are successful with lower echelon personnel, problems are created in their relations to superiors. This results in such comments as: "What I have learned now I will do. But please teach it to my superior."

Several American owned companies operating in Western Germany have attempted to install American-type courses in executive development and management training. When these involve American ideas concern-

ing span of control and responsibility and authority, they do not seem to be successful. It is doubtful, however, that a course taught by a German psychologist becomes exactly what the American psychologist who planned the course expected it to be. German ideas about personnel policies are not always American ideas.

Accident prevention. Several psychologists are investigating motivating factors in causes of accidents. One problem, for example, is a study of the effects on the accident rate of social laws providing 90% sick and accident pay. Another is a study of personality of people who handle explosives.

The increase in highway traffic has created problems. An American driving on German highways gets an impression of extreme aggressiveness of German drivers. Apparently German psychologists are aware of this, too, for there are in progress studies of driver attitudes in relation to accidents. This interest in accident prevention carries over into both selection and training: the selection of drivers and attempts to identify accident-prone workers, and in training in accident prevention.

Productivity. Physiological and environmental factors as they relate to production are the concern of physiologists and work methods people. A few psychologists are involved in this kind of productivity research, principally at the Max Planck institute.

There is definitely an awareness among active industrial psychologists of personal and social factors in relation to production. This, again, is reflected in both selection and training: attempts to select workers on the basis of personality factors that are related to performance, and training in leadership techniques with increased production as at least one of the goals.

CONCLUSION

Having taken a look at industrial psychology in two countries, what generalizations emerge?

In Britain, industrial psychologists are doing much less than they know how to do. The actual putting into practice in industry of psychological principles lags behind research. This would seem to be somewhat the opposite of the state of affairs in the United States where psychologists often seem to be applying in industry much that has not yet been demonstrated as valid and where applied psychology in industry seems often to have gone beyond research in the field.

The British have developed, on paper, a system of industrial psychology which looks quite impressive to the reader of the professional literature. There is little to criticize in the picture they have presented except that it possesses too little substance.

There are good reasons for this lack of substance. There is a shortage of professional personnel of all

sorts, a shortage aggravated by a draining off to the economically more favorable climate of the dollar countries. Psychology has been so affected not a little. And there is the shortage resultant from not producing new psychologists through the years of a long war. Nor has British educational tradition been favorable to the development of a field so new and so application oriented as is the psychology known in America.

But to say that it has not been done is not to say that it can not or will not be accomplished. The British may look at American applied psychology with the disdain, tempered by guarded admiration, of a stay-at-home elder brother looking on the accomplishments of the younger brother who wandered afar; yet they may eventually accept and adopt much of what they consider worthwhile of the younger brother's accomplishments. It may be that a distinctive British industrial psychology will develop that will include much that is good from American industrial psychology, refined through research and improved through the winnowing out of many of the crudities inherent in almost anything that has grown so rapidly. British industrial psychology, though small in the number of people involved, has the stability of unbroken tradition.

But this is not so in Germany. The younger generation of West German psychologists have had to build on the only foundation they have: the work of the prewar psychologists of their own country. But there was a long period devoid of experimental work, devoid of organized psychology of any kind. This was a period of isolation from the ideas of much of the rest of the world. The older German psychologists do not all read English with ease. Those who do have not always had English and American professional literature to read. Their psychology tends to be insular.

The younger men are avid for knowledge. Some of them have gone to England for a year of study. A very few have been to America. They read and speak English very well. They are reading all of the American literature they can get their hands on.

For a time there were no journals through which industrial psychologists could exchange ideas. *Psychologische Rundschau* was established in 1950. Its contents include a few articles in the field of industrial psychology. A newer journal, *Psychologie und Praxis*, established in 1956, is distinctly oriented toward psychological applications in industry and management in general.

A handbook of industrial psychology is in preparation. It is a joint project of several industrial psychologists under the editorship of Arthur Mayer at Mannheim.

In both America and Britain there is a tendency for psychologists who have worked in industry a few

years to begin to identify with management more than with the profession of psychology. This appears to be much less so in Germany where the psychologist is proud of his profession and very proudly remains identified with it.

In concluding his survey of European industrial psychology more than 35 years ago, Viteles (1923) quotes Moede (1922) as follows:

Summarizing the actual accomplishments to date in industrial psychology, there is ground both for daring optimism and doubting pessimism, but in considering the youth of this subject and the relatively short time during which basic experiments have been carried out, one is drawn to the side of optimism.

Moede is dead. So are most of his colleagues of that time. German industrial psychology died completely in World War II. But it has been reborn, and the optimism persists.

The bonds that tie together American, British, and German industrial psychologists are not entirely historical, although these are strong. The present-day bonds of common interests, common problems, and common aspects of methodology could do with a bit of strengthening. All concerned would gain.

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Psychology in the News

Tests . . .

When one reads a headline these days about **URGES MORE TESTS OR PROTESTS FURTHER TESTS**, one never knows whether the news story beneath it will be discussing nuclear fallout or the important pros and cons of psychological tests. Both of these matters seem to threaten future generations, and both of them generate controversy quite gritty with true and false statistics.

The November issue of the *Journal of the National Education Association* has a special section, no less than 17 pages, devoted to the general subject **HAS TESTING GONE TOO FAR?** This question, which make a nice headline on the cover of the NEA journal, seems to be like similar questions one sees every day on the newsstands. Editors find that it sells magazines if one runs across the cover a yellow band with big black letters to ask something like: **IS THE COMPACT CAR A MENACE TO AMERICA'S LOVERS' LANE TRADITION? OR ARE DISCARDED POP BOTTLE CAPS RUINING OUR BAREFOOT CHILDREN'S FEET?** Most magazines see to it that their articles never quite answer such queries, and the NEA journal at no point becomes absolutely explicit, not in black on white and certainly not in black on yellow. It is every teacher for himself in deciding whether testing has gone too far.

This series includes an article on "Test Anxiety" done by Seymour B. Sarason and an article by H. Gerthorn Morgan called "What Is Effective Evaluation?" A pro and con discussion of a "Nationwide Testing" program is done as a centerpiece in this group, with Robert L. Ebel of Educational Testing Service upholding the affirmative:

Would increased participation in nationwide achievement-testing programs indicate that the schools are being overrun with testing and in danger of being taken over by the testers? Not if the program is voluntary. Not if it is under the control of educational leaders. Not if less useful testing is restricted. . . .

All of us who ask for better schools and better teachers owe it to those who pay the bills to do the best we can to show that the money is being well spent. Effective use of a good voluntary nationwide testing program would be a long step in this direction.

Frederick M. Raubinger, State Commissioner of Education for New Jersey, concludes in the last three paragraphs of his article:

The amount of testing which is being done in our schools at present is so great that it interferes with teaching, simply because it takes up too much time, makes proctors out of teachers, and gives the false impression that education is largely a matter of taking and passing examinations.

Tests have their uses, but each school should determine the tests and have complete freedom to choose those that suit its purpose. Testing agencies should exist to serve the schools, not to set educational policy. In no event, should they exercise undue influence or attempt to determine what should be taught.

Undoubtedly, the testing tail is presently wagging the educational dog. It's time for the dog to start wagging its own tail.

From down in Atlanta comes an article in the Sunday magazine section of the *Atlanta Journal and Constitution*. The title is "How Fair Are the IQ Tests?" One hopes they are fairer than the newspaper's cameraman was to Warren G. Finley, Assistant Superintendent of Schools, photographed as he purportedly was "studying quiz scores." Joseph E. Moore, Professor at Carnegie Tech, John R. Hills, Director of Testing and Guidance with the University System Board of Regents, and Sam C. Webb, acting Department Chairman at Emory University, are other psychologists mentioned in this piece, which concludes that tests are fair if used fairly and not taken by themselves to sum up an entire personality.

This is reaching a bit far back, but also in our file is an extensive treatment of testing by *Newsweek* of July 20, 1959. Their headline question was **TESTING: CAN EVERYONE BE PIGEONHOLED?** The writer's conclusions were perhaps implicit in the two final paragraphs of the story:

There is often a big gap between the ideals of the test people and the way tests are used in actual practice. Unfortunately, we have very little control over this. Any inexperienced counselor or personnel manager, if he gets his hands on test results, can use them in most foolish ways.

Despite these cautions, however, it seems unreasonable to look back to the good old days when informal interviews, personal hunch, or a letter of recommendation could place

a man in a school or a job. Testing, with its cool reliance in the democracy of scores and numbers, is here to stay.

* * *

Referral Service for Would-Be Suicides . . .

A center for community-wide referrals of suicidal persons has been set up in Los Angeles as a five-year project supported by the National Institute of Mental Health. Broad purpose of the project, staffed by psychologists, psychiatrists, and social workers, is to seek precise and scientific information on suicide. Principal investigators Edwin S. Shneidman, of the University of Southern California, and Norman L. Farberow, of the Veterans Administration, have collected records on all suicides or attempted suicides in Los Angeles County during the last decade and have established a new phrase: "Psychological Inquest."

Educational Television . . .

The University of Michigan radio station is planning two new series on human behavior and the problems of world peace. Glen D. Phillips, producer-interviewer with the station, will interview about one hundred persons here and in Canada,

including Richard C. Snyder, Harold Guetzkow, Theodore F. Lentz, and Karl Menninger.

Great Neck Studied in Survey . . .

Psychological knowledge was applied, and seemed to be helpful, in the course of a school fight in Great Neck, Long Island. The citizens of Great Neck voted down a \$10,000,000 school budget and then passed one almost as large less than two months later.

The Great Neck Education Association undertook to find out what citizens knew about schools. A survey was prepared by Alfred B. Udow, Research Director of Lennen & Newell, an advertising agency.

Although the salary of the school superintendent was described as a "heated issue," half the respondents in the survey did not know what it was. Estimates were from \$6,500 to \$40,000—the actual salary is \$24,500. Teachers' annual pay was estimated from \$4,000 to \$10,000—generally teachers are paid \$7,000 in Great Neck. People thought school board members might get from \$5,000 to \$18,000 a year—actually they get no pay.

—MICHAEL AMRINE

Psychology in the States

Front Row Center

Readers of Allen Drury's *Advise and Consent* may have become hard-bitten experts on what can go on behind committee doors or even in open session. May we take them and you into at least one fairly typical APA meeting by way of showing that it cannot—or at least does not seem to—happen here.

Prologue. The scene is the APA Central Office, which plays host to many an APA committee; the dramatis personae, the Board of Professional Affairs (with several Central Office staff members in the wings); the time, November 21–22, 1959. There are no houselights to dim; neither have the lines been memorized nor the programs rigged. And this is roughly the way the story goes.

Setting. The script seems to write itself after a fashion. Born just two years ago, the Board of Professional Affairs—long since dubbed BPA—has found itself partly able to freewheel. But only partly. Its fall meeting usually allows at least some opportunity for conjecturing about where the profession is headed; who (or which of the Fates) is charting its course; and whether the direction is fortunate or ominous, fluid or unalterable. Come spring, the luxury of philosophizing about destiny (whether manifest or latent) needs to yield to the unenviable job of reviewing the work of some dozen committees and almost as many representatives to other organizations, not to mention issues of divers kinds. This year the two acts of the drama seemed to merge somewhat, yielding effects we should like to report for your information.

Plot. If BPA has developed any soap-opera-like routine at this stage, it is not evident from a look at the program notes for this meeting. The themes could be called “professional” ones, we suppose; although discussion of Science Fairs, standards for research in hypnosis, and the construction of a certification examination seem hardly to stop there. Again, the roles psychologists play as expert witnesses in legal hearings and court cases are not easily squeezed into any particular category. And the matter of relating to education and educators

takes as much account of the research contributions psychology can make as it does of the simple working out of a coexistence formula. At any rate, these were among the themes running through the November cycle.

Action. Without further prelude, let us get to the action, or actions, trying as best we can to build in the necessary continuity.

Relations with Other Professions. We do not know whether Riesman would call it other-directedness, but the board did give considerable thought to our relations with other professions. In some cases, we profess to love one another, yet the marriage bears little fruit; this is the situation BPA confronts in our relations with education or with religion. In others, simple rapprochement still remains to be reached (at least on the organizational level); here relations with our psychiatric colleagues are a case in point. All three situations occupied the board at its meeting.

BPA considered the joint report (a third draft) of the committees of the two APAs with reference to relations between psychology and psychiatry. The two committees have agreed on the present version; it remains for the associations themselves to endorse the report prior to its joint publication. Accordingly, it was voted by BPA to recommend that the Board of Directors approve the present version of the joint report of the committees on relations between psychology and psychiatry.

In the matter of relations with education, the situation, if less charged, was also less definite. On prior recommendation of this board, the Committee on Relations between Psychology and Education had been disestablished. As the responsible agent, BPA was particularly concerned lest the action be misunderstood. It had been in no wise intended as a slight to the committee's past efforts, still less as a minimizing of the importance of relating to education. The committee structure had not, however, proved the proper vehicle. Hence, BPA felt the more constrained to explore a variety of alternative mechanisms for communicating and collaborating with education and educators. This it

did at the present meeting; and here are but a few of the many possibilities it saw at the national and state levels, inside and outside APA:

1. Attracting more of the competent psychologists to the advisory panels of the Office of Education
2. Becoming more actively involved in the implementation of the provisions of several Titles in the National Defense Education Act
3. Bringing to the attention of psychologists typical projects receiving government and foundation support
4. Continuing active participation in such agency-creating conferences as those dealing with a Council for Research in Education
5. Encouraging surveys by one or several APA Divisions with regard to the nature and extent of research being done by psychologists in the area of education
6. Analyzing psychologists' attitudes toward and motivations for doing research in education
7. Reactivating and extending earlier Education and Training Board studies of the teaching of psychology in professional schools.

Lest these and other ideas remain at the talking stage, BPA agreed to an early appointment of a not very mysteriously labeled "Task Force E."

The area of religion presented no fewer difficulties, as the board saw it. Equally dubious about the feasibility of relating to such an amorphous area through the instrumentality of a committee, BPA voted to recommend the disestablishment of the Committee on Relations between Psychology and Religion. Again, it emphasized vigorously the need for paying heed to the relations between the two fields, suggesting that here too the use of task forces charged with more circumscribed and clearly defined missions might be by far the better approach. Finally, the very real problems raised by the committee with respect to training in pastoral counseling might well be ones at which the Education and Training Board could be asked to look.

Science, Hypnosis, and Test Construction. A whole cluster of issues reminded the board (if it needed reminding) that science is everywhere. Indeed, it was at the suggestion of the Board of Scientific Affairs that BPA devoted its attention to Science Fairs and the recruitment opportunities they present. When Barbara Neal, National Science Fair winner, was invited to the Cincinnati convention as a guest of APA, her prize winning

psychological exhibit suggested to many that there, by the grace of state psychological associations, might go many more bright young prospective psychologists. By the time this column appears in print, state association presidents will have received from the APA Central Office, at the suggestion of BPA, materials which will, hopefully, get this project off its launching pad.

Next came the question of the scientific reputability of those who engage in research in hypnosis as well as in its teaching and practice. The issue had been raised with the board by psychologists mindful of the need for top-level standards in an area demanding a high order of competence and an impeccable sense of social responsibility. BPA carefully reviewed information which had been gathered on the Society for Clinical and Experimental Hypnosis, among others, as well as on the related American Board of Examiners in Psychological Hypnosis. In view of the ABEPP-like standards of the latter, it was recommended that APA members who are ABEPH Diplomates be appropriately listed in the APA *Directory* beginning with the 1961 edition.

Also concerned with matters of examination and standards was the report BPA heard on the Conference of State Boards of Examiners, which had been held during the two days prior to its own meeting. At this Chicago conference the Committee on State Examination Procedures met with 12 representatives of statutory boards of examiners, 4 representatives of nonstatutory boards, the BPA Chairman, the Chairman of the Committee on Legislation, and the APA Legislative Consultant. The conference recommended, and BPA concurred, that an interim committee be established to plan an appropriate workshop for the 1960 APA Annual Meeting. Among the several products of the conference was a proposal that state boards of examiners be canvassed with respect to (a) the desirability of creating an Association of State Examination Boards and (b) the suitability of developing or monitoring a common examination.

State Associations and Directories. As is their wont, the state associations were seen as playing one or another role in almost everything from approaches to the Better Business Bureau to the gathering of information on the present state of forensic psychology. We shall forego mention of some of the projects in which the associations will

subsequently find themselves involved anyway. In view of interest previously expressed in one development, however, we hasten to mention that the BPA Ad Hoc Committee on Insurance Coverage for Psychological Services will shortly meet jointly with the Committee on Paramedical Relations of the Health Insurance Council.

May we, then, go on to a more tangible project which promises to involve the state associations in greater or less degree in the near future. It has to do with directories, and perhaps we can structure the situation fairly nondirectively since the proposed relationship is in its very early phases.

BPA makes a habit of inviting others to share its stage when the latter could more pointedly speak to the issues than could the board itself. Such was the case at the present meeting, where BPA had been requested to consider the problems faced by the American Board for Psychological Services. The latter agency, it will be remembered, was saddled with the responsibility of producing a *Directory of American Psychological Services*, a comprehensive and representative compendium of competent agencies and individuals offering psychological services to the public. Karl F. Heiser and Wendell S. Dysinger, President and Secretary-Treasurer of ABPS, respectively, were invited to share their experiences and exchange their views with the members of the board. At this point we cannot report any final actions; we do, however, offer the following under the heading of "straws in the wind" or some equally evasive caption.

Ironically enough, the board felt, at this stage APA suffers not from a dearth but from an oversupply of directories, each with its peculiar shortcomings. The APA *Directory* "overlists" with respect to such features as "private practice," an entry now to be had for the asking (although more rigorous criteria for such listing will grow out of the recommendations of the Committee on Private Practice). By contrast, the ABPS directory "underlists," omitting many of the better individuals and agencies, primarily because they have not applied for evaluation. ABEPP, for its part, indicates little in its directory beyond the name and locality of its diplomates, two-thirds of whom appear to be unavailable for psychological services to

the public. Finally, the state association directories, while in some instances laudable beginnings, differ widely in character and adequacy and in some cases are not yet available.

Is there room here, indeed need, for a sharing of effort, BPA wondered? The state associations have the resources, ABPS has the background; the state associations seem frankly to welcome guidance, ABPS has developed a body of experience in the area of evaluation; state association directories should preferably be at least reasonably comparable, ABPS is ideally situated to serve as a kind of intermediate mechanism.

We hasten to point out that there were no secret agreements at this meeting, nor even an attempt to sell (or soft sell) a prefabricated directory plan. It can be safely stated, though, that ABPS will, at its next meeting, take such glimmerings as the above seriously under advisement. And if we may venture a prediction, it would be that state associations will hear more about the business of directory production and their role as producers. Very likely the problem will become the theme of an appropriate workshop at the 1960 APA Annual Meeting.

Epilogue. We have dwelt at some length on the little drama which unfolds when an APA board meets (invariably on a week end). Partly this is because BPA abhors the privacy of the smoke filled room, supports the doctrine of open communication, and welcomes whatever free associations reporting such as this may help elicit. And partly it is because we have hoped to show that the scenes in this APA play (or work) do not rest simply on what some may regard as the eternal triangle: legislation, relations with psychiatry, and private practice. Something more goes on. And that something more, we venture to say, reflects the fact that a board, by whatever name, is still a collection of psychologists; and psychologists, under whatever roof and of whatever ilk, find many a motif to dwell upon together.

—M. CURTIS LANGHORNE

Chairman

Board of Professional Affairs

ERASMUS L. HOCH

Administrative Officer

State and Professional Affairs

Notes and News

In 1960, the following **APA reports and publications** are scheduled to appear in the indicated issues of the *American Psychologist*:

PUBLICATION	ISSUE
Call for Papers and Symposia for the APA Annual Convention	January
New Members and Associates of APA	January
Internships for Doctoral Training in Clinical Psychology	January
General Arrangements for the APA Annual Convention	April
Applicants for Fellow Status	April
Doctoral Training Programs in Clinical and Counseling Psychology	June
Program of the APA Annual Convention	July
Proceedings of regional psychological associations	September
Address of the President of APA	December
Annual Report of the Executive Secretary	December
Proceedings of the APA Annual Business Meeting	December
Officers, Boards, Committees, and Representatives of APA	December
APA Distinguished Scientific Contribution Awards	December
American Psychological Foundation Awards	December
Educational Facilities and Financial Assistance for Graduate Students	December

Copy for these publications should be received in the APA Publications Office by the fifteenth of the third month preceding the month of issue in order for each publication to appear in the scheduled issue.

To appear in a given issue of "Notes and News," copy for each entry must be received in the APA Publications Office by the first of the month preceding the month of issue. For example, if it is desired that an entry appear in a March issue of "Notes and News," it is necessary, in order to meet press deadlines, for copy to be received by February 1.

The APA has received an advisory opinion from the Civil Aeronautics Board for a proposed **APA Charter Flight** to the sixteenth International Congress of Psychology in Bonn, Germany, July 31-August 6, 1960. Tentative plans call for departure from New York City to Cologne on Friday evening,

July 29, 1960 and return from Paris to New York on Thursday, August 26, 1960. Estimated round trip fare (based on 1958 tariffs) will be approximately \$280.00 per person. Arrangements are being made with a major airline. Passengers will move as a unit in both directions—one-way fares are not permitted. In compliance with CAB regulations, participation in the APA Charter Flight will be limited to those APA members (and immediate, accompanying family) who are participating in the congress program (reading papers, etc.) or who have been appointed congress delegates by their state psychological associations. Administrative Officer for the APA Charter Flight is Henry P. David (1 Jill Lane; Trenton 8, New Jersey). Requests for information should be accompanied by two self-addressed, stamped, legal sized envelopes.

The American Board of Examiners in Professional Psychology, Inc., on November 13, 1959, administered its eleventh written examination to 124 candidates at 56 examining centers. ABEPP wishes to express its appreciation to the following Diplomates who served as proctors for its eleventh written examination:

John P. Anderson	Rhea R. Hilkevitch
Seth Arsenian	Philip S. Holzman
Harvey R. Austrin	Reuben S. Horlick
Katharine M. Banham	Albert Hunsicker
Emanuel M. Berger	Thelma Hunt
Earl C. Brown	Walter F. Johnson, Jr.
Thelma E. Brown	Marvin W. Kahn
Arnold Buss	Barbara A. Kirk
Linda Carter	Virginia Kirk
Katharine Cobb	Irwin J. Knopf
Louise W. Cureton	Robert P. Larsen
Paul R. Dingman	Ivan N. McCollom
Arthur A. Dole	Fletcher McCord
Seymour Epstein	Fred McKinney
Edmond F. Erwin	Joseph M. Masling
Paul J. Fay	Joseph D. Matarazzo
Maurice H. Fouracre	Stanley Moldawsky
Mildred Gebhard	Leola E. Neal
Leonard Goodstein	I. R. Obenchain
Harold A. Goolishian	David H. Orr
Leon Gorlow	Mervin Patterson
Iona C. Hamlett	Leslie Phillips

Jessie L. Rhulman
Anne M. Ritter
Dorothy W. Seago
David Shapiro
Agnes A. Sharp
Daniel E. Sheer

John A. Stern
Harley R. Wideman
Irving Wolf
Phillip Worchel
J. Lewis Yager
Marvin Zuckerman

The American Board of Examiners in Professional Psychology, Inc. is now receiving applications for admission to its 1960 written examination. Applications will be reviewed at its midyear meeting in February and again at its annual meeting in August. The final date for making application for written examination in 1960 is May 1. ABEPP has prepared a statement entitled *Policies and Procedures*. This pamphlet gives specific information on requirements for candidacy, fields of certification, the nature of acceptable qualifying experience, and evaluative procedures, including written and oral examinations and policies governing these examinations. A copy of this pamphlet may be obtained by writing to: Noble H. Kelley, Secretary-Treasurer; Department of Psychology, Southern Illinois University; Carbondale, Illinois.

Robert H. Mathewson, of the Division of Teacher Education, Colleges of the City of New York, represented the APA at the Centennial Anniversary Convocation of the Cooper Union for the Advancement of Science and Art in New York City on November 2, 1959.

Eckhard H. Hess represented the APA at the Special Convocation commemorating the centennial of the publication of Charles Darwin's *The Origin of Species* held at the University of Chicago on November 26, 1959.

E. E. Arnstein, of Jerusalem, Israel, died on July 27, 1959.

J. Lawrence Broderick, of New York City, died on October 28, 1959.

Dean Farnsworth, of the Office of Naval Research (London), died on December 27, 1959.

Richard J. Kingham, of Philadelphia, Pennsylvania, died on November 13, 1959.

Tom A. Lamke, of Iowa State Teachers College, died on August 2, 1959.

Joe L. Lawson, Jr., of Alexandria, Louisiana, died on July 14, 1959.

Thomas B. Meadows, of Chattanooga, Tennessee, died in November 1959.

Ruth Muriel Meulendyke, of Old Saybrook, Connecticut, died on October 17, 1959.

Cecil Vernon Millard, of East Lansing, Michigan, died in January 1959.

Bela Mittelman, of New York City, died on October 2, 1959.

Thomas H. Mooney, of Bradfordwoods Boro, Pennsylvania, died on August 4, 1959.

David C. Rogers, of Middletown, Connecticut, died on October 13, 1959.

Edward C. Tolman, of the University of California, Berkeley, died on November 19, 1959.

Robert L. Weislogel, the late Director of the Western Office of the American Institute for Research, died on October 28, 1959.

Ida R. Wightman, of Harrisburg, Pennsylvania, died on October 7, 1959.

Ronald F. Wilson, of Houston, Texas, died on September 29, 1959.

John E. Winter, of Morgantown, West Virginia, died in 1959.

Editor's Note. *New appointments of only members of the APA are announced in this section of "Notes and News." Space limitations make it necessary to exclude announcements: of promotions within an agency when no essential change in function or location is implied, of lists of personnel when no appointment additional to or different from what already appears in the APA Directory (now an annual publication) is involved, and of the appointments of personnel who are not members of the APA. Under these policies, this section is a kind of interim directory in which the entries describe the new appointments or relocations of members of the APA.*

William F. Banaghan and Cameron W. Meredith have accepted positions at Southern Illinois University (Alton Campus).

Selwyn W. Becker has been appointed Assistant Professor of Psychology in the Graduate School of Business at the University of Chicago.

Roger Bellows, on leave from Rutgers University, is Visiting Professor this year at the University of Minnesota.

John E. Bentley retired from American University after 36 years of service; he is now Visiting Professor of Psychology at Florida Southern College.

James E. Birren will be a Visiting Professor in the Psychology Department and the Committee on Human Development at the University of Chicago during the winter quarter.

The following have joined the staff of the Division of Psychological Services of the Board of Education, Montgomery County, Maryland:

B. Miller Eves, formerly with the Board of Public Instruction, Volusia County, Florida

Jane S. Harris, formerly with the United States Public Health Service

Ernest C. Young, formerly with Anne Arundel County Boards of Education and Health

J. Marshall Brown, Head of the Department of Psychology at Lafayette College, was elected a representative on the Easton-Forks-Easton Area Joint School Board.

Roger V. Burton, formerly at Harvard University, has joined the staff of the Laboratory of Socio-environmental Studies at the National Institute of Mental Health. **Marian Radke-Yarrow**, Chief of the Section on Social Developmental and Family Studies in the laboratory, has been freed of her administrative responsibilities for a year to study current approaches to research on family-child relationships.

Richard G. Cannicott has resigned from the Central State Griffin Memorial Hospital and from the University of Oklahoma to accept a position at Los Angeles State College (not Los Angeles City College as erroneously reported on page 715 of the November 1959 issue).

James F. Carey has joined the San Francisco office of McKinsey & Company, Inc. as a member of the personnel relations group.

Frederick Carleton, formerly of Science Research Associates, has rejoined the staff of Richardson, Bellows, Henry & Co., Inc. as Manager of the New Orleans office.

Truman M. Cheney, formerly State Supervisor of Guidance and Counseling in Montana, is now Associate Professor of Education in charge of Counselor Training at the University of Nevada.

The Devereux Foundation has awarded a Pre-doctoral Research Fellowship to **Sherwood B. Chorost**, of the University of Texas.

David Rigler has resigned from the Philadelphia Child Guidance Clinic to be Senior Psychologist on the staff of the Child Research Council at the University of Colorado School of Medicine. **Edward Clifford** is also a member of the staff.

In the Department of Psychology at New York University:

Charles N. Cofer, formerly at the University of Maryland, has been appointed Professor of Psychology and Director of Graduate Studies.

Jacob Cohen, formerly at the Montrose Veterans Administration Hospital, has been appointed Associate Professor of Psychology.

Courtney and Company of Philadelphia announces the following additions to its research staff:

W. Crawford Clark, formerly of Project Michigan

John Eberhard, formerly of Misericordia Hospital

Robert Kerle, formerly with the Philadelphia Jewish Employment and Vocational Service

Richard B. Cravens, formerly with the Hawaii Department of Health, has been appointed a Consultant in Psychology in the Division of Mental Diseases, Jefferson City, Missouri.

Irving S. Feldman has resigned from the New Jersey State Diagnostic Center to be Administrative Director and Chief Psychologist of the newly organized Mental Health Clinic of Ocean County, Toms River, New Jersey.

William J. Fielder, formerly with the Aurora Public Schools, has joined the staff of the Child Study and Consultation Service, Phoenix Public Schools, Arizona.

Violet Franks, formerly with the National Association for Mental Health in London, has been appointed Senior Clinical Psychologist in the Psychology Service and Research Center of the Neuro-Psychiatric Institute, Princeton, New Jersey.

Sheldon L. Freud has been designated the permanent United States Air Force Representative on the Mental Health Study Section, Division of Research Grants, National Institute of Health.

Robert S. Czeh and **Jacob R. Lebsack** have joined the Defense Systems Department of the General Electric Company in Syracuse, New York,

as human factors specialists. The staff also includes **Hugh L. LaMonaca** and **Melvin Sorcher**.

William T. Bourke, **Goodwin Watson** (part-time), and **Albert S. Thompson** (consultant) are members of the staff of the recently opened New York City office of **Glaser, Snowden & Associates**.

Arnold P. Goldstein, formerly at Pennsylvania State University, has joined the staff of the **Stanton Clinic**, University of Pittsburgh School of Medicine.

Robert B. Gorrell, formerly with the Jefferson County Mental Health Clinic, has joined the Milwaukee staff of **Humber, Mundie & McClary**.

Jerome M. Judd has been appointed an Account Associate by **Walter V. Clarke Associates, Inc.** **Kermit R. Hasler** is currently on leave of absence to complete his doctoral studies at New York University.

The Howard Crosby Warren Medal for 1959 was awarded by the Society of Experimental Psychologists to **Harry Helson** "for his development of the concept of adaptation-level"

Samuel Hirschenfang, formerly at the Jewish Chronic Disease Hospital, has been appointed Clinical Psychologist in the new Hearing and Speech Clinic at the Kings County Hospital Center, Brooklyn, New York.

Bette Holzberg, formerly with the Larchmont-Mamaroneck School System, is now Director of Psychological Services at the Westchester Children's Remedial Center, Yonkers, New York.

The Meritorious Civilian Service Award was presented by the Department of the Army to **Joseph H. Kanner** for his contributions to the development of television as a military training and communications medium.

Saul Kasman, formerly with the Illinois Public Aid Commission, has joined the staff of the Chicago Veterans Administration Regional Office as Counseling Psychologist.

William E. Kendall has been named staff psychologist in charge of developmental activities at the Psychological Corporation, New York City.

Vita Krall, formerly at the Topeka State Hospital, has accepted a position as Clinical Psychologist in the Outpatient Department and as Acting Chief Psychologist in the Kansas Neurological Institute (Topeka), a new hospital for mentally retarded children.

Saul C. Kupferman has completed his work on the OVR project at the United Cerebral Palsy Association and has joined the staff of the Jewish Vocational Service, Los Angeles, as Director of Counseling Services.

William B. Lewis, clinical psychologist at the Paso Robles School for Boys, has been honored with the first quarterly Outstanding Employee Award of the California Youth Authority. The award read:

In recognition of outstanding contributions to the program of the department, including the development of improved methods of treatment for more seriously disturbed wards.

Walter M. Lifton, Associate Professor of Education at the University of Illinois, has been appointed Director of Guidance Publications and Services at Science Research Associates, Chicago. **Khossrow Mohandessi** has joined the Research and Development Department of SRA.

Elizabeth Faulk and **Mark Rudnick** have joined the staff of the Menninger Foundation as two-year postdoctoral Fellows in clinical psychology. **Julia Fraknoi** is now in the second year of her postdoctoral fellowship.

Martin L. Hoffman has been appointed Editor of the *Merrill-Palmer Quarterly of Behavior and Development*. APA members who are serving as Consulting Editors are: **Joseph Adelson**, University of Michigan; **Wesley Allin Smith**, Harvard University; **Richard Q. Bell**, National Institute of Mental Health; **Robert D. Hess**, University of Chicago; **Jerome Kagan**, Fels Research Institute; **Dorothy Lee**, Harvard University; **Ronald Lippitt**, University of Michigan; **Norman A. Polansky**, Western Reserve University; and **Daniel A. Prescott**, University of Maryland. The journal publishes articles representing the various disciplines bearing on human development, personality, and social relations.

Archer L. Michael has resigned from the Milwaukee County Hospital for Mental Diseases to join the staff of the System Development Corporation, Santa Monica, California.

Harriett B. Moore, Associate Director of Social Research, Inc., is currently serving as Director of the SRI's German affiliate, the Institut für Absatzpsychologie, in Hamburg.

Rolf E. Muuss has resigned from the State University of Iowa and has been appointed Associate Professor of Education and Child Development at Goucher College, Towson, Maryland.

John G. Darley, Gardner Murphy, and Otto Klineberg have been elected to the Professional Committee of the National Association for Mental Health. **Ralph Ojemann** has been elected Chairman of the Education Committee and a member of the Executive Committee.

Oscar A. Parsons, formerly at Duke University, has become Professor of Medical Psychology and Head of the Division of Medical Psychology in the Department of Psychiatry, Neurology, and Behavioral Sciences at the University of Oklahoma Medical Center.

Robert B. Payne has left the Arctic Aeromedical Laboratory to become Deputy Director of Medical Research at the School of Aviation Medicine, Brooks Air Force Base, Texas.

Sheldon B. Peizer has resigned from the United States Naval Hospital in Memphis to become Clinical Psychologist at the Federal Correctional Institution, Ashland, Kentucky.

For a six-month period beginning in February 1960, **Edwin Terry Prothro** will be on leave from the American University of Beirut to be a Research Fellow in the Center for Middle Eastern Studies at Harvard University.

Sherman C. Raffel, formerly with the Montgomery County Health Department, has been appointed Assistant Professor of Clinical Psychology and Director of the Psychology Section, Department of Psychiatry, University of Alabama Medical Center.

Jerome Rosenblum has accepted the position of acting Chief Psychologist at the Wayne-Medina-Holmes Guidance Center, Wooster, Ohio.

Jerome Sattler, formerly of Osawatomie State Hospital, has been appointed Associate Professor of Psychology at Fort Hays Kansas State College.

Sam C. Scher, formerly at the Twin Cities Mental Health Clinic, has accepted a position with the Hamm Memorial Psychiatric Clinic in St. Paul, Minnesota.

Carleton F. Scofield has been appointed Associate Director of the new Center for Applied Social Research at New York University.

Sidney Siegel, of Pennsylvania State University, co-author of the manuscript "Bargaining and Group Decision Making: Experiments in Bilateral Monopoly," was one of the winners of the 1959 Academy Monograph Prize in the social sciences awarded by the American Academy of Arts and Sciences.

In the Psychology Department at the State Hospital, Salem, Oregon:

New members of the staff are **William H. Banaka**, from the University of Houston; **Walter M. Coulter**, from the University of Maryland; **Merritt C. Cushing**, formerly at the Beatty Memorial Hospital; **Sidney I. Dean**, from the Mills Clinic; and **Irwin D. Nahinsky**, formerly with the Oregon Study for the Rehabilitation of Mental Patients.

Carol L. Bowdish, from the University of Portland, and **Vernon Dean Kiewer**, from the University of Oregon, are interns in clinical psychology.

Consultants are **Roy E. Buehler**, **Walter G. Klopfer**, and **Joseph D. Matarazzo**.

W. Porter Swift, formerly with the Missile Test Project at Cape Canaveral, has joined the staff of Edward N. Hay & Associates to be in charge of the Philadelphia Office.

John R. Taylor, of the Montana Center for Cerebral Palsy and Handicapped Children, has also been appointed Associate Professor in Psychology at Rocky Mountain College.

George G. Thompson, formerly at Syracuse University, has joined the staff of Ohio State University as Professor of Psychology.

Lawrence Tirnauer, formerly at Pennsylvania State University, has joined the Psychology Department staff at Saint Elizabeths Hospital, Washington, D. C.

Charles A. Ullmann, Director of Management Institutes for the United States Civil Service Commission, was given an award by the Training Officers Conference in Washington, D. C. for outstanding service in the field of employee training. The award cited his leadership in the promotion and development of management intern training programs for federal employees and the operation of interagency training on the management of financial and scientific programs.

In the Department of Psychology at the University of Oregon:

New members of the staff are **Thomas Gordon Johnson**, Associate Professor of Marketing; **Lloyd Lewis Lovell**, Assistant Professor of Education; **Harry A. Shoemaker**, Assistant Professor of Psychology; and **James H. Straughan**, Assistant Professor of Psychology.

Richard A. Littman has returned from sabbatical leave spent in the Child Research Branch, National Institute of Mental Health, Bethesda, Maryland.

Norman D. Sundberg is on leave in the Institute of Personality Assessment Research, University of California, on a Senior Stipend from the United States Public Health Service. **Leona E. Tyler** is on special assignment with the United States Office of Education to prepare a report on the Counseling and Guidance Institutes held in the summer of 1959 under provision of the National Defense Education Act.

The following personnel changes have occurred in Psychology Services, Department of Medicine and Surgery, Veterans Administration:

Allan Barclay, a graduate of the VA Psychology Training Program, Washington University, has been appointed to the Clinical Psychology Staff, VA Mental Hygiene Clinic, St. Louis, Missouri.

Janeth Carpenter has resigned from the Psychology Service, VA Center (Gulfport Division), Biloxi, Mississippi.

Paul B. Carpenter, a graduate of the VA Psychology Training Program, Florida State University, has been appointed to the Psychology Service, VA Center (Gulfport Division), Biloxi, Mississippi.

Jonathan Cummings has transferred from the Hines VA Hospital to the position of Chief, Psychology Service, VA Center, Fargo, North Dakota.

James G. Drasgow has been appointed to the Vocational Counseling Staff, VA Hospital, Buffalo, New York.

Louis Feigenbaum has resigned from the Clinical Psychology Staff, VA Hospital, Bronx, New York.

William R. Grove has resigned from the Psychology Service, VA Hospital (Leech Farm Road), Pittsburgh, Pennsylvania.

Roy M. Hamlin has been appointed to the Psychology Service, VA Hospital, Danville, Illinois.

John J. Hoffman has been appointed to the Clinical Psychology Staff, VA Hospital, Buffalo, New York.

Ann Q. Hozier has been appointed to the Psychology Staff, VA Mental Hygiene Clinic, Los Angeles, California.

Marvin Hyman has resigned from the Detroit VA Mental Hygiene Clinic to accept a position with the Receiving Hospital, Wayne State University Medical School, Detroit, Michigan.

Wilma Knox has transferred from the Biloxi VA Center to the Psychology Service, VA Hospital, Sheridan, Wyoming.

Bernard H. Light has resigned from the Psychology Service, VA Hospital, Palo Alto, California.

Fred Lipschitz has been appointed to the Clinical Psychology Staff, VA Hospital, Richmond, Virginia.

Roy C. Long, a graduate of the VA Psychology Training Program, University of Texas, has been appointed to the Psychology Staff, VA Hospital, Waco, Texas.

Paul Mandeville has resigned from the Psychology Service, VA Hospital, Knoxville, Iowa.

Max L. Marshall has transferred from the Memphis VA Hospital to the Psychology Service, VA Hospital, Murfreesboro, Tennessee.

Bernard Mikol, a graduate of the VA Psychology Training Program, Michigan State University, has been appointed to the Clinical Psychology Staff, VA Mental Hygiene Clinic, Detroit, Michigan.

Albert T. Milam, a graduate of the VA Psychology Training Program, University of Oklahoma, has been appointed to the Psychology Service, VA Hospital, Tuskegee, Alabama.

Carl Newman has resigned from the Psychology Service, VA Hospital, Brooklyn, New York.

Katherine Ni has resigned from the Perry Point VA Hospital to accept a position with the Division of Neurology and Blindness, National Institutes of Health.

Hal W. Osborn has been appointed to the Psychology Service, VA Hospital, Salt Lake City, Utah.

Richard Paynter, who has been Chief Clinical Psychologist, VA Mental Hygiene Clinic, New York City, since 1948, has retired from federal government service.

Sanford Rosenzweig, a graduate of the VA Psychology Training Program, Indiana University, has been appointed to the Psychology Service, VA Hospital, Montrose, New York.

Alfred Rustebakke has transferred from the Minneapolis VA Hospital to the Clinical Psychology Service, VA Mental Hygiene Clinic, Albuquerque, New Mexico.

Herman C. Salzberg, a graduate of the VA Psychology Training Program, University of Tennessee, has been appointed to the Psychology Service, VA Hospital, Augusta, Georgia.

Sterling D. Schultz has resigned from the Psychology Service, VA Hospital, Palo Alto, California.

Harold J. Segel has transferred from the Butler VA Hospital to the Psychology Service, VA Hospital, Sepulveda, California.

Charlyne T. Seymour has accepted a position with the Louisiana Evaluation Center for Exceptional Children, New Orleans, Louisiana, while on a one-year leave of absence from the Clinical Psychology Service, VA Hospital, Long Beach, California.

Rowland Shephard has transferred from the Perry Point VA Hospital to the Clinical Psychology Staff, VA Hospital, Bedford, Massachusetts.

Norman Tallent has transferred from the Kecoughtan VA Center to the Psychology Service, VA Hospital, Northampton, Massachusetts.

Frank G. Verdicchio has transferred from the Richmond VA Hospital to the position of Chief Clinical Psychologist, VA Hospital, Huntington, West Virginia.

John Vitale has transferred from the Sepulveda VA Hospital to the Psychology Service, VA Hospital, Palo Alto, California.

J. Frank Whiting has resigned from the Psychology Service, VA Hospital (Leech Farm Road), Pittsburgh, Pennsylvania.

Fred H. Wright has transferred from the Tuscaloosa VA Hospital to the Psychology Service, VA Center (Gulfport Division), Biloxi, Mississippi.

Stuart L. Weissman, of Yeshiva University, has been awarded a fellowship from the United States Public Health Service.

George T. Wilcox has resigned from the Dayton Adult Psychiatric Clinic to accept the position of Chief Psychologist at the Lake County Mental Health Clinic, Painesville, Ohio.

M. Erik Wright, of the University of Kansas, has been awarded a Fulbright Lectureship in Mental Health to the University of Western Australia for the 1960-61 academic year. **Beatrice Wright** will also be at the University of Western Australia.

Frederick A. Zehrer is now Chief of the Office of Educational Services at the Army Medical Service School (Fort Sam Houston, Texas), a newly created office involving educational advisor duties.

William Zielonka has resigned as acting Director of the Psychoanalytic Studies Institute in Philadelphia to assume responsibilities as psychologist with Hunt Foods and Industries, Inc., Fullerton, California.

The following rosters of officers have been announced:

Metropolitan New York Association for Applied Psychology

President: Frederick Hauser
Vice-President: Sheldon Zalkind
Secretary: Edith MacLauchlan
Treasurer: Robert I. Dawson

Queens-Nassau Psychologists in Psychotherapeutic Practice

President: Ephraim Friend
President-elect: Muriel Margolis
Secretary-Treasurer: Kenneth Helfant

Richmond Psychological Association

President: Charles Hart Westbrook
Vice-President: Harold E. Paine
Secretary-Treasurer: Walter Frank Johnson, Jr.

Southern Electroencephalographic Society

President: Don L. Winfield

As a membership organization of some 288 colleges, the **College Entrance Examination Board** sponsors a research program dealing with many aspects of the school to college transition: from the development of an examination program for predicting college success to the exploration of differences in college environments. In addition to the development and validation of the entrance test program, ways are being sought to improve the prediction of college success by studying the non-intellective factors related to college performance: investigations on ways of defining college success, the process of choosing a college, instruments for measuring student attitudes and aspirations, and the effect of socioeconomic backgrounds on test scores and college achievement. There is increasing interest in studies of the characteristics of colleges and high schools which may affect the relationship between predictors and criteria of college success—characteristics ranging from physical facilities and administrative organization to modes of communication between students and faculty or the actual and perceived goals, values, and pressures of the college. Research reports on recent studies are currently available to social scientists and educators. Please address requests, comments, or questions about the College Board research program to: Ann K. Pasanella, Research Associate; College Entrance Examination Board; 425 West 117 Street; New York 27, New York.

The **Devereux Foundation** announces that 1960-61 fellowships in clinical, counseling, and

school psychology are available; apply to: Henry Platt, Director of Psychological Training; Devereux Foundation, Institute for Research and Training; Devon, Pennsylvania.

The **Hofheimer Prize** of \$1,500 is awarded annually by the American Psychiatric Association for an outstanding research contribution in the field of psychiatry or mental hygiene which has been published within a three-year period up to the date of the award. This competition is open to citizens of the United States and Canada who were not over 40 years of age at the time the study was submitted for publication, or to a research group whose medium age does not exceed 40 years. The next award will be made at the Annual Meeting of the association in May 1960. Entries submitted to the Prize Board before March 1, 1960 will be considered. Eight copies of each entry and data concerning age and citizenship should be sent to: David A. Hamburg, Chairman, Hofheimer Prize Board; National Institute of Mental Health; Bethesda 14, Maryland.

The Harry M. Cassidy Memorial Research Fund in the School of Social Work at the University of Toronto has awarded a grant to Howard Jones, of the University of Leicester, England, for preparation of a book describing a research project on the development of **alcoholism** with particular study of social factors.

The Office of Naval Research has made a grant to Charles N. Cofer, of New York University, for a three-year study of the role of **association processes** in verbal behavior and verbal learning.

The College of Education of the State University of Iowa announces the availability of a monograph "**Critical Counseling Behavior in Rehabilitation Settings**" by Marceline Jaques. It reports findings obtained by using the critical incident technique with 404 rehabilitation counselors in 20 states. This investigation and the publication of the final report was supported, in part, by a research grant from the Office of Vocational Rehabilitation, Department of Health, Education, and Welfare. Counselors in the state-federal programs and other agencies or persons on OVR mailing lists will secure copies of this report directly from OVR. Copies

can be obtained without cost by writing to: Project Director; College of Education, State University of Iowa; Iowa City, Iowa.

A renewal grant has been made by the Office of Naval Research to F. Kenneth Berrien and his associates at Rutgers University to continue their research into **group stability**; the effect of disturbances by other groups and persons on the continued life of the group is the target of the present study.

The National Science Foundation has awarded an Antarctic research grant to William M. Smith to accompany the Victoria Land Traverse as a team member while studying **individual and group behavior** during the long, arduous, and isolated trek.

The Opinion Research Group in the General Planning & Research Division of the Prudential Insurance Company in Newark, New Jersey, has formulated a five-year program for systematically applying the social sciences to the problems of **marketing life insurance**. The techniques of survey research and of small group research are among those being used in the first stages of the program.

The National Science Foundation has awarded the following grants in psychobiology:

M. Alpern, University of Michigan, "Studies of Contrast Phenomena"

J. D. Birch, University of Michigan, "Role of Extinction in Reversal Learning"

C. A. Boneau, Duke University, "Empirical Study of the *t* Test"

R. A. Butler, University of Chicago, "Effects of Brain Damage on Responsiveness to Visual and Auditory Incentives"

B. A. Campbell, Princeton University, "Studies on Aversive and Reinforcing Properties of Stimuli"

N. E. Collias, University of California, Los Angeles, "The Analysis of Nest Building in Weaverbirds"

A. L. Diamond, University of Hawaii, "Simultaneous Brightness Contrast and the Brightness Response"

S. E. Glickman, Northwestern University, "Studies of Animal Behavior"

A. D. Hasler, University of Wisconsin, "Environmental Influences on Fish Behavior"

J. E. Hochberg, Cornell University, "Dimensions of Form Perception"

W. E. Jeffrey, University of California, Los Angeles, "Research on Discrimination Learning"

R. F. Johnston, University of Kansas "Comparative Behavior of American Ground Doves"

J. W. Kaswan, University of California, Los Angeles, "Factors in Visual Perception"

D. L. LaBerge, University of Minnesota, "Studies in Stimulus Generalization"

C. Landis, New York State Psychiatric Institute, "Studies of Flicker Fusion Determinants"

L. P. Lipsitt, Brown University, "Studies of Discrimination Learning"

R. A. McCleary, University of Michigan, "Studies of Interocular Transfer"

J. T. Marshall, University of Arizona, "Research on Speciation"

E. C. Raney, Cornell University, "Behavior of *Cyprinidae*"

W. F. Reynolds, Rutgers University, "Role of Secondary Reinforcement in Learning"

D. A. Riley, University of California, "Studies of Transposition Behavior"

E. D. Schubert, Cleveland Hearing and Speech Center, "Interaural Temporal Disparity"

C. E. Sherrick, Jr., Washington University, "Study of Vibratory Stimuli"

R. L. Solomon, Harvard University, "Experiments on Aversive Autonomic Conditioning"

J. S. Stamm, Institute of Living, "Effects of Cortical Stimulation on Learning and Retention"

H. W. Stevenson, University of Minnesota, "Probability Learning"

P. Teitelbaum, University of Pennsylvania, "Effect of Hypothalamic Lesions on Behavior"

L. S. Tsai, Tulane University, "Interspecies Studies of Behavior"

H. Wallach, Swarthmore College, "Study of Perceptual Learning"

E. O. Wilson, Harvard University, "Behavior of *Dolichoderinae*"

The objective of the research program in the Arts Center of the Boston University School of Fine and Applied Arts is "to pursue the place of the arts in contemporary and emerging American life by the use of social science insights and methodology." The first issue of the research division's monthly bulletin, *The Arts and the Behavioral Sciences*, has been published; interested persons can be placed on the bulletin mailing list by contacting the Arts Center. The bulletin includes reports of ongoing research programs, announcements of special events related to the arts, and lists and reviews of new works in the field.

The Institute of International Education (1 East 67 Street; New York 21, New York) has established a Research Information Unit to offer information and guidance to organizations and individuals concerned with research in the international

educational exchange of persons movement, educational systems around the world, and the teaching of English as a foreign language.

The Psychology and the Sociology Departments at the University of Chicago have been awarded funds by the National Institutes of Health for a graduate training program in social psychology. Fred L. Strodtbeck will direct the training program.

A study designed by the Superior and Talented Student Program of the North Central Association of Colleges and Secondary Schools (57 West Grand Avenue; Chicago 10, Illinois) to identify superior and talented students in high school, to encourage them to achieve well in academically demanding courses, and to motivate them to continue their education beyond the high school has been given further financial support by the Carnegie Corporation of New York.

The University of Michigan has received a grant from the National Science Foundation to make a study of the undergraduate curriculum in psychology. Following a survey of present curricula, to be conducted this year, a conference will be held at Ann Arbor in the summer of 1960. Personnel of the conference will include Lawrence E. Cole, William A. Hunt, Robert Leeper, W. J. McKeachie, John E. Milholland, and Wilbert Ray.

The Surgeon General's Office of the United States Army has awarded additional contracts to the Boston Research Laboratories (133 West Oregon Street; Milwaukee 4, Wisconsin) for vibration research designed to determine the effects of vertical vibration on operators of mobile vehicles. The project is under the direction of Melvin A. Schmitz.

The first supplement to the *Index of Psychoanalytic Writings* is now being prepared. Letters have been sent to approximately 4,000 APA members, asking for a list of their writings of psychoanalytic interest. The recipients of these letters are urged to return the reply sheets. Others who have published material of psychoanalytic interest, but did not receive one of the queries, may write to: Alexander Grinstein; 18466 Wildemere Avenue; Detroit 21, Michigan.

The Rehabilitation Center of the Buffalo Children's Hospital held a workshop, October 23-24,

1959, on "Psychological and Educational Evaluation of Children with Special Handicaps."

A two-day seminar in **existential therapy** was conducted by Adrian L. van Kaam, November 12-13, 1959, at the Veterans Administration Hospital, Lebanon, Pennsylvania.

The Los Angeles Society of Clinical Psychologists held a **Conference on Postdoctoral Training** on November 15, 1959. Issues discussed included: responsibility for training new PhDs, preparation for ABEPP diplomas, the functions of a "certifying" agency in offering training, criteria for selecting individual supervisors.

The topic of the November 18 meeting of the Association for Psychiatric Treatment of Offenders (8 East 97 Street; New York 29, New York) was "**Authority and Treatment.**"

The 1960 workshop in the **Rorschach technique** of personality diagnosis and other projective techniques as used with children, sponsored by the Claremont Graduate School and the Los Angeles Children's Hospital, was held on January 4-15, 1960 under the direction of Bruno Klopfer.

The first Annual Meeting of the **Academy of Religion and Mental Health** was held on January 13-15, 1960 in New York City. For further information, write to: Kay Overholt; ARMH; 2 East 103 Street; New York 29, New York.

The National Rehabilitation Association (1025 Vermont Avenue, N.W.; Washington 5, D. C.) and the International Society for the Welfare of Cripples sponsored a seminar on January 28-29, 1960 in Washington, D. C. for representatives of organizations interested in international programs for the **rehabilitation of the physically handicapped.**

The Community Guidance Service is sponsoring workshops in **psychoanalytic therapy**, starting in January 1960. For further information, write to: Bertram Pollens, Workshop Director; Community Guidance Service; 140 West 58 Street; New York 19, New York.

The Association for the Advancement of Psychoanalysis (220 West 98 Street; New York 25, New York) announces the eighth annual **Karen Horney Lecture** by David McK. Rioch on "Recent Contributions of Neuropsychiatric Research to the Theory and Practice of Psychotherapy" on March 23, 1960 at the New York Academy of Medicine.

The twelfth Annual Conference of the South-

western Institute of Radio Engineers (P. O. Box 22331; Houston 27, Texas) will be held on April 20-22, 1960 in Houston, Texas; one of the sessions will be on applications of electronics to medicine, biology, and the behavioral sciences.

The Committee on Diagnostic Reading Tests is sponsoring an Annual Workshop on **Reading Research** in April 1960 in Philadelphia, Pennsylvania. For further information, write to: CDRT; Kingscote Apt. 36; 419 West 119 Street; New York 17, New York.

The Survey Research Center of the University of Michigan will hold its thirteenth annual summer institute on **Survey Research Techniques** on July 18-August 13, 1960. For further information, write to: SRC, University of Michigan; Ann Arbor, Michigan.

The National Training Laboratories this coming summer will again conduct experimental and training laboratories in **human relations training** and in community leadership training. For further information, write to: NTL; 1201 Sixteenth Street, N.W.; Washington 6, D. C.

The twenty-fourth Annual Meeting of the **Japanese Psychological Association** will be held on July 17-20, 1960 at the University of Tokyo. For further information, write to: JPA Annual Meeting Committee; Department of Psychology, University of Tokyo; Bunkyo-ku, Tokyo, Japan.

Horst Finkemeyer, social psychologist on the staff of the Institut für Absatzpsychologie in Hamburg, Germany, will be associated with Social Research, Inc. (145 East Ohio Street; Chicago 11, Illinois) for a six-month period to become acquainted with American methods of studying consumer behavior.

Books for Asian Students, a project of the Asia Foundation, reports that by April 1959 over one million books and a quarter of a million journals had been sent to thousands of recipients in Asia. Program materials are of a selective nature and meet rigid criteria. A new and ambitious book program has been developed for the Federation of Malaya, which is served by 15 teacher-training institutions. The foundation has been asked to furnish a basic library of 2,500 volumes for each of these

schools; titles in education, geography, history, arts and crafts, and reference books are needed. *The program therefore requests contributions for this project*, as well as other new programs and growing commitments for books in other Asian countries. In addition to the titles needed for the Malaya project, there is a continued demand for items in every category at the university and college level, in good condition, published in 1948 or after, and works by standard authors regardless of date. Contributions can be sent to: Books for Asian Students;

21 Drumm Street; San Francisco 11, California. Transportation costs from the donor to San Francisco and thence to Asia will be borne by the foundation for substantial shipments.

Magazines for Friendship, Inc. (Occidental College; Los Angeles 41, California) urges readers to send used copies of their journals to foreign public libraries or universities. The agency will supply information on mailing procedures and on libraries having particular need for scientific literature.

Convention Calendar

American Psychological Association: September 1-7, 1960; Chicago, Illinois

For information, write to:

Janice P. Fish
American Psychological Association
1333 Sixteenth Street, N.W.
Washington 6, D. C.

Southwestern Psychological Association: March 24-26, 1960; Galveston, Texas

For information, write to:

Beatrix A. Cobb, Secretary
Department of Psychology
Texas Technological College
Lubbock, Texas

Southeastern Psychological Association: March 31-April 2, 1960; Atlanta, Georgia

For information, write to:

Susan W. Gray
Box 30
George Peabody College for Teachers
Nashville 12, Tennessee

Southern Society for Philosophy and Psychology: April 14-16, 1960; Biloxi, Mississippi

For information, write to:

Dan R. Kenshalo
Florida State University
Tallahassee, Florida

Eastern Psychological Association: April 15-16, 1960; New York, New York

For information, write to:

Carl H. Rush
P. O. Box 252
Glenbrook, Connecticut

Western Psychological Association: April 21-23, 1960; San Jose, California

For information, write to:

Brant Clark
Department of Psychology
San Jose State College
San Jose 14, California

Midwestern Psychological Association: April 28-30, 1960; St. Louis, Missouri

For information, write to:

I. E. Farber, Secretary-Treasurer
Midwestern Psychological Association
Department of Psychology
State University of Iowa
Iowa City, Iowa

Rocky Mountain Psychological Association: May 5-7, 1960; Glenwood Springs, Colorado

For information, write to:

William H. Brown
Department of Psychiatry
University of Utah College of Medicine
156 Westminster Avenue
Salt Lake City 15, Utah

Inter-Society Color Council: April 11-12, 1960; Philadelphia, Pennsylvania

For information, write to:

Ralph M. Evans, Secretary
Inter-Society Color Council
Color Technology Division, Building 65
Eastman Kodak Company
Rochester 4, New York

International Conventions

International Congress of Anthropological and Ethnological Sciences: July 31-August 7, 1960; Paris, France

For information, write to:

Henri Vallois, Directeur
Musée de l'Homme, Palais de Chaillot
Place du Trocadéro
Paris 16e, France

International Union of Scientific Psychology, Sixteenth International Congress of Psychology: July 31-August 6, 1960; Cologne, Germany

For information, write to:

Otto Klineberg
Department of Psychology
Columbia University
New York 27, New York

International Association of Gerontology, Fifth International Congress on Gerontology: August 7-12, 1960; San Francisco, California

For information, write to:

Louis Kuplan
P. O. Box 2103
Sacramento 10, California

World Federation for Mental Health, Thirteenth Annual Meeting: August 7-12, 1960; Edinburgh, Scotland

For information, write to:

Secretary-General
World Federation for Mental Health
19 Manchester Street
London, W.1, England

International Congress of Individual Psychology: August 28-September 1, 1960; Vienna, Austria

For information, write to:

W. Spiel, Secretary
Lazarettgasse 14
Vienna 9, Austria

International Pharmaceutical Federation, Twentieth International Congress of the Pharmaceutical Sciences: August 28-September 2, 1960; Copenhagen, Denmark

For information, write to:

International Pharmaceutical Federation
11, Alexanderstraat
The Hague, Netherlands

International Society for the Welfare of Cripples, Eighth World Congress: August 28-September 2, 1960; New York, New York

For information, write to:

Donald V. Wilson, Secretary-General
701 First Avenue
New York 17, New York

International Council of Group Psychotherapy, Third Congress: Summer 1960; Paris, France

For information, write to:

Wellman J. Warner
Department of Sociology and Anthropology
Graduate School of Arts and Sciences
New York University
New York 3, New York

Interamerican Society of Psychology, Seventh Congress: December 26-30, 1960; Havana, Cuba

For information, write to:

G. M. Gilbert
Department of Psychology
Long Island University
Brooklyn 1, New York

International Association of Applied Psychology, Fourteenth Congress: August 21-26, 1961; Copenhagen, Denmark

For information, write to:

E. Tranekjaer Rasmussen
Psychological Laboratory
Studiestraede 6, Studiegaarden
Copenhagen K, Denmark

International Federation for Medical Psychotherapy, Fifth International Congress of Psychotherapy: August 21-26, 1961; Vienna, Austria

For information, write to:

H. K. Fierz, Hon. Secretary
Committee for the International Congresses of Psychotherapy
Kreuzlingen, Switzerland

World Federation for Mental Health, Sixth International Congress on Mental Health: August 30-September 6, 1961; Paris, France

For information, write to:

Secretary-General
World Federation for Mental Health
19 Manchester Street
London, W.1, England

CALL FOR PAPERS AND SYMPOSIA
SIXTY-EIGHTH ANNUAL CONVENTION OF THE
AMERICAN PSYCHOLOGICAL ASSOCIATION

Sherman and Morrison Hotels

Chicago, Illinois, September 1-7, 1960

APA CONVENTION PROGRAM COMMITTEE

I. INTRODUCTION

THE APA Convention Program Committee announces a Call for Papers and Symposia for the 1960 Annual Convention to be held in Chicago, Illinois.

Considerable experience and tradition tend to be incorporated in the organization of our Annual Conventions. Because the purposes of our conventions are many and varied, different demands and requirements must be taken into account in our attempts to arrange the program in the best interests of all members and groups. Certain procedures, regulations, and deadlines have to be established to coordinate our convention affairs. This coordination is necessarily complex. Members therefore must adhere to certain specifications in presenting their ideas, proposals, and materials for the program of the convention.

The relevant rules and procedural instructions are

given in this call. Please read these carefully. In order to make the 1960 Annual Convention the success you desire, we will need your cooperation in following the procedures outlined below. **Of particular interest are the deadlines, the forms for symposium proposals, the form for abstracts of contributed papers, and the proper persons to receive your correspondence.** The pertinent references have been collected in the boxed summary on this page for your convenience. Information about convention arrangements will appear in the April issue of the *American Psychologist*. Forms concerning convention registration, hotel reservations, APA Buffet Luncheon reservation, child care facilities, and a call for volunteer services are included at the end of this call.

II. KINDS OF PROGRAMS AND SESSIONS

The meetings regularly contain many kinds of programs and sessions: research papers, symposia, group

Symposia. If you wish to initiate or participate in a symposium, check these sections:

- III. Qualifications for Participation
- IV. Procedures for Proposing Symposia
 - A. Initiating Symposia
 - B. Form of Symposium Proposals
 - C. Special Divisional Rules for Symposia
- V. D. Audio-Visual Aids
- X. Summary of Deadlines
- XII. Program Chairmen

Additional Programs. If you wish to initiate or participate in another type of program, check these sections:

- III. Qualifications for Participation
- V. D. Audio-Visual Aids
- VI. Additional Programs
 - A. Initiation of Other Types of Programs
 - B. Divisional Plans for Other Types of Programs
- X. Summary of Deadlines
- XII. Program Chairmen

Individual Papers. If you wish to submit an individual paper, check these sections:

- III. Qualifications for Participation
- V. Procedures for Submitting Papers
 - A. Research Papers
 - B. Other Papers
 - C. Form of Abstracts and Summaries
 - D. Audio-Visual Aids
 - E. Where to Send Abstracts and Summaries
 - F. Special Divisional Policies for Individual Papers
- X. Summary of Deadlines
- XII. Program Chairmen

Other Activities. For information on other activities, check these sections:

- II. Kinds of Programs and Sessions
- VII. Business Meetings and Special Sessions
- VIII. Film Presentations
- IX. Exhibits
- X. Summary of Deadlines
- XI. APA Convention Chairmen

TABLE 1
CONVENTION SCHEDULE

September 1 Thursday	September 2 Friday	September 3 Saturday	September 4 Sunday	September 5 Monday	September 6 Tuesday	September 7 Wednesday	
		Teaching (M)	A P A D A Y	Teaching (M)			
Clinical (S)				Clinical (S)	General (M)		
School (M)	Public Service (M)	Educational (M)		Experimental (M)			
Personality and Social (S)				Industrial and Business (S)			
Developmental (M)	Maturity & Old Age (M)	Counseling (M)		Esthetics (S)		Military (M)	
Consulting (S)		SPSSI (S)		SPSSI (S)			
	NCPAD (S)			Measurement (S)			
				Engineering (M)			
		Psi Chi (S)		Psi Chi (S)			
APA Board of Professional Affairs (M)				APA Board of Professional Affairs (M)			

discussions, addresses, business meetings, film sessions, as well as other more informal events such as reunions, dinners, and social hours.

Table 1 presents the tentative convention schedule. Members may plan their attendance with the expectation that the final program will bear close resemblance to this schedule, though minor revisions may be necessary. The hotel (S = Sherman, M = Morrison) in which the majority of each division's program will be scheduled is indicated in parentheses after each division code name in Table 1; these entries are tentative, but changes from one hotel to the other will not be made unless necessary.

The July *American Psychologist* will contain four major sections outlining the details of the Annual Convention. The first section will contain the substantive program including such events as papers, symposia, and invited addresses. The second section will contain a listing of the meetings of the APA boards and committees, divisions, and Psi Chi, including their presidential addresses, business meetings, and social functions. The third section will contain, at the discretion of the APA Convention Program Committee, announcements of the pre-convention and post-convention meetings and workshops and of meetings of organizations not officially

affiliated with APA. The fourth section will contain announcements and notes from the Convention Manager describing details of local arrangements, recommendations for making your own convention plans, and suggestions for your better enjoyment of the convention and its facilities.

Divisions and Psi Chi will screen and select their own substantive programs for the convention. Other groups wishing to have substantive programs must be sponsored by one of the divisions or by Psi Chi, with the exception of their presidential addresses. Requests for scheduling presidential addresses for non-APA groups must be directed to the Chairman of the APA Convention Program Committee. Programs which do not readily fit into a divisional or affiliated structure may be suggested to the APA Convention Program Committee and may be scheduled at the discretion of the committee. All requests for space and time must be submitted to a division, to Psi Chi, or to the APA Convention Program Committee before **March 1** for papers, and before **March 15** for fully organized symposia and other types of substantive programs. These deadlines must be observed if the event is to be announced in the July *American Psychologist*.

It is obviously to the advantage of a non-APA group

to propose its substantive program to a division because divisional sponsorship places the session in the substantive section of the APA program and permits abstracts or details to be published about the program. If a non-APA group applies directly to the APA Convention Program Committee to have its session approved and announced, the announcement could at best appear only as a brief note in the third section of the printed program.

The Chairman of the APA Convention Program Committee receives all requests for the scheduling of non-substantive program activities such as business meetings, reunions, luncheons, dinners, social hours, and headquarters space. He should also receive any proposals and suggestions for programs and events of interest to all APA members. For divisions and Psi Chi, all details of requests and proposals for nonsubstantive events must be received by the APA Convention Program Committee before **April 15**—and for non-APA groups, by **March 15**—in order to insure announcement in the July *American Psychologist*. After these APA Convention Program Committee deadlines, all requests for nonsubstantive events are to be sent to the Associate Convention Manager.

In summary, proposals for papers are to be sent before **March 1**, and fully organized symposia and other substantive program items are to be sent before **March 15**, directly to a divisional program chairman. Non-APA groups proposing substantive programs that do not fit into divisional structure may submit such proposals before **March 15** to the APA Convention Program Committee Chairman. Nonsubstantive programs must be submitted by divisions and Psi Chi before **April 15** and by non-APA groups before **March 15** directly to the Chairman of the APA Convention Program Committee if they are to appear in the July *American Psychologist*.

III. QUALIFICATIONS FOR PARTICIPATION

A. Limits of Individual Participation

Over the past several years, the APA Convention Program Committee has developed certain policies for the limits of individual participation in the Annual Convention program. These policies are designed to ensure the widest possible participation by APA members and also to prevent troublesome conflicts in the time schedule. The policy is that each member may present no more than one paper and that each member may, in addition, participate in no more than one additional session such as a symposium or a discussion group.

B. Symposia and Invited Addresses

Any group of members of the APA may present a symposium, provided that the proposal has been ac-

cepted by a divisional program committee. It is not necessary for the persons in the group to be members of the division to which a symposium is proposed.

A divisional program committee or the APA Convention Program Committee may invite distinguished nonmembers to contribute to the program as special speakers or as participants in symposia. Acceptance of a distinguished speaker or of a symposium proposal by a divisional program committee or by the APA Convention Program Committee constitutes the required sponsorship of nonmember participants.

C. Papers

Any member of the APA may read a paper, provided that it has been accepted by a divisional program committee. It is not necessary to be a member of the division to which the paper is submitted.

A nonmember of the APA (Foreign Affiliate, member of the Student Journal Group, etc.) may read a paper, provided that an APA member is co-author or sponsors the paper and that the paper is accepted by a divisional program committee. The APA member who agrees to sponsor a nonmember must submit the abstract of the nonmember's paper to the chairman of the appropriate divisional program committee with an accompanying brief description of the nonmember's scientific or professional qualifications.

IV. PROCEDURES FOR PROPOSING SYMPOSIA

Results of many formal and informal inquiries of those attending the Annual Conventions clearly indicate that symposia are the preferred type of program. Organization of symposia is, therefore, encouraged by the APA Convention Program Committee. All symposia must be planned to last 50 minutes, an hour and 50 minutes, or 2 hours and 50 minutes because of the way in which the scheduling of convention affairs will be organized.

A. Initiating Symposia

A member may submit a fully organized symposium for a divisional program committee's consideration; such symposia should be submitted to a divisional program chairman before **March 15**. Also, any member of the APA may suggest a symposium topic to the chairman of the appropriate divisional program committee; such proposals must be made before **March 1**.

Symposia may also be initiated by any divisional program committee or committees, or by a divisional program committee in cooperation with a non-APA group. Organization of such symposia is encouraged by the APA Convention Program Committee.

B. Form of Symposium Proposals

1. *Final form.* The following outline should be used in submitting a fully organized symposium in order that appropriate acknowledgments will appear in the July *American Psychologist*:

Title of Symposium
Chairman, Institution of Chairman

Participants:
Name, Institution of Each Participant. Title of Contribution.

Discussants (if any): Name, Institution of Each Discussant.

Five copies of the final organized symposium must be submitted to a divisional program chairman by **March 15**. A list of names, institutions, and addresses of the chairman and the participants must accompany the proposal. Slide requirements, if any, must be submitted with the symposium (see Section V-D below).

2. *Form for suggested symposia.* When a member suggests but does not organize a symposium, he should indicate the title and significance of the topic and list the name, institution, and address of the proposed chairman and of the other participants and discussants. Such suggestions should be sent to a divisional program chairman before **March 1** to allow time for planning.

C. Special Divisional Rules for Symposia

Division 1. General Psychology. This Division is interested in co-sponsoring symposia with other divisions and invites early correspondence on topics of joint interest.

Divisions 3, 7, and 8. Experimental Psychology, Developmental Psychology, and Personality and Social Psychology. These Divisions require that proposals for member organized symposia include a 100-word general statement (five copies) about the nature and importance of the symposium topic plus a 300-word summary (five copies) of each presentation. These materials are for the use of these divisional program committees and will not be printed in the program.

Divisions 9, 14, 15, and 19. Society for the Psychological Study of Social Issues, Industrial and Business Psychology, Educational Psychology, and Military Psychology. These Divisions require that a proposal for a member organized symposium be accompanied by a 200-word statement (five copies) about the nature and importance of the symposium topic.

Division 22. National Council on Psychological Aspects of Disability. This Division will organize two symposia. Members are invited to submit specific suggestions, including the proposed title, a statement about the nature and importance of the topic, and a list of names and addresses of appropriate discussants.

V. PROCEDURES FOR SUBMITTING PAPERS

Unless otherwise indicated under Special Divisional Policies (see Paragraph F below), four 10-minute papers will be scheduled for each 50-minute session. In instances of multiple authorship, the person whose name is listed first will be expected to read the paper.

A paper previously read at any regional meeting may not be read at the Annual Convention unless it is a substantial elaboration (additional findings, etc.) of a preliminary report. More than one paper reporting highly similar findings from a cooperative project may not be read at the Annual Convention.

Abstracts printed in the *American Psychologist* are limited to 100 words. However, it is recognized that more detailed information will be needed by the divisional program committees for use in the selection of papers. The procedures for research papers and other papers are described below.

A. Research Papers

Each author of a research paper must submit a 100-word abstract (1 copy), for publication if the paper is accepted, and also a 300-word summary (4 copies) for the division committee program consideration. If the author desires, tables presenting results may be submitted with the 300-word summary. Not more than one page of tables should be submitted. All the data should have been collected and the analysis completed at the time the abstract and the summary are submitted to a divisional program chairman.

B. Other Papers

Theoretical papers, case studies, and like material are acceptable for the program. The 100-word abstract of a nonexperimental paper must, however, be accompanied by the complete manuscript (4 copies).

C. Form of Abstracts and Summaries

All abstracts and summaries must be typed on one side of the paper only, double spaced throughout, and on 8½" × 11" paper. The 100-word abstract and the first copy of the 300-word summary must be on bond paper, not on onion skin or other thin paper.

1. *The 100-word abstract.* Abstracts should be concerned with content and theory, rather than with method and technique (unless the paper is essentially methodological). Abstracts must be limited in length to 100 words (not counting title, author, and institution). Abstracts should not contain tables, drawings, footnotes, or bibliographic entries.

The following outline must be followed in preparing the abstract:

Title of Paper

Author(s), Institution(s)

(Sponsor, if any)

Text of Abstract (not to exceed 100 words)

Do not underline or type anything with all capital letters. The typewritten abstract should be checked and proofread carefully, since it will be printed in the form in which it is submitted.

2. *The 300-word summary.* The text of the summary will normally include a statement of the problem, subjects used, procedure, results, and conclusions.

Summaries must be limited in length to 300 words (not counting title, author, and institution). The 300-word summary may be accompanied by not more than one page of supplementary tables, drawings, footnotes, etc.

The form for submitting the 300-word summary should be exactly the same as for the 100-word abstract except, of course, for the longer text.

Four copies of the 300-word summary and the supplementary tables, etc. are required. Author, institution, address, and sponsor should appear only on the first two copies of the summary. Slide requirements, if any, must be stated on this summary (see Section V-D below).

D. Audio-Visual Aids

Members are urged to consider carefully the advantages of duplicated handouts of graphic and tabular material over presentation by slide projection, particularly if professional facilities are not available to produce the slides. Provision will be made, however, for presenting slides when this is required:

1. Where one or more participants on a fully organized symposium require slides, a statement must be submitted for each participant, regarding the size of the slides that are to be projected.

2. Authors must note, on the 300-word summary in the case of research papers, or as a footnote on the title page in the case of theoretical papers submitted in full, the size of any slides that are to be projected.

3. If slides are used, standard lantern slides (3 1/4" x 4") are preferred since projectors for 2" x 2" slides are in less adequate supply.

Facilities will also be provided for the use of motion picture films, tape recorders, and other audio-visual aids as required. Statements of requirements for such devices must be submitted in the same manner as for slides. For film or other audio-visual aid presentations not made in association with specific symposia or papers, see Section VIII.

Under the regulations of the hotels and the projectionists' union, only union qualified projectionists may operate any piece of audio-visual equipment at any

meeting; all such equipment will be provided by the hotels. Members will not be allowed to use their own equipment.

No provision for audio-visual aids can be made unless requested as indicated above.

E. Where to Send Abstracts and Summaries

Copies of the abstract and the summary of a paper should be sent to one of the divisional program chairmen listed in Section XII before **March 1**. A paper may be submitted to only one division. The member need not belong to that division.

F. Special Divisional Policies for Individual Papers

Some of the divisions have special policies. A member planning to submit a paper to one of the following divisions should note carefully its special policy.

Division 1. General Psychology. This Division will not present programs of individual research papers. Its program will consist of symposia, invited addresses, and theoretical papers. Papers (30 to 40 minutes) that summarize and organize particular fields are especially desired.

Division 3. Experimental Psychology. This Division requests that 2 additional copies of the 100-word abstract be submitted.

Divisions 7 and 8. Developmental Psychology and Personality and Social Psychology. At the discretion of these Division Program Committees, some of the individual papers will be accompanied by discussion by invited participants. The 10-minute time limit will be maintained for individual papers in these sessions.

Division 9. Society for the Psychological Study of Social Issues. This Division's program will consist largely of symposia and discussion groups. All individual research papers which might ordinarily be considered to fall in the domain of Division 9 are to be sent instead to Division 8, Division of Personality and Social Psychology (see above).

Divisions 12 and 14. Clinical Psychology and Industrial and Business Psychology. The time allotted for individual papers will vary from session to session and will be determined in each case after the abstracts and the summaries have been received.

Division 19. Military Psychology. This Division will sponsor individual research papers, and a limited number of invited addresses. Abstracts for papers should indicate whether the author prefers to give a 5-minute summary or a 12-minute paper.

Division 21. Society of Engineering Psychologists. In addition to the customary symposia and 12-minute research papers, two innovations will be tried: 5-minute summaries of research findings, and 25-minute

reports which summarize a comprehensive study or series of experiments. Abstracts submitted should indicate preferred time to be allotted and which other time limits are acceptable.

Division 22. National Council on Psychological Aspects of Disability. This Division invites the submission of research and theoretical papers dealing with the psychological aspects of disability, with reference to both children and adults.

VI. ADDITIONAL PROGRAMS

A. Initiation of Other Types of Programs

In the past the programs of the APA Annual Conventions have been composed primarily of individual papers and of symposia. It was indicated earlier that the organization of symposia should be considered as one of the major features of the convention. In the interests of broadening our convention programs, it is suggested that the divisional program committees develop ideas for new kinds of programs. Members are urged to send suggestions for new types of programs to the appropriate divisional program committee chairman. Such special sessions should be suggested to a chairman before **March 1**. Procedures for initiating new types of programs should follow in general the procedures for initiating symposia (see Section IV).

B. Divisional Plans for Other Types of Programs

Division 8. Personality and Social Psychology. This Division invites members to hold small discussion groups on specialized topics, which will be announced in the program. Groups of from 10 to 25 persons are anticipated. If preliminary circulation of manuscripts is planned, details should be included in the request for such a meeting. The name of the person responsible for each group will be printed in the program.

Division 9. Society for the Psychological Study of Social Issues. This Division invites suggestions from its members for small discussion groups on special topics. The suggested format includes: (1) title; (2) a statement of no more than 200 words indicating (a) the relevance of the specialized topic to the field of psychology encompassed by Division 9, (b) the contribution such a discussion group would make to the program of Division 9 as well as an indication of some of the main themes and ideas that the group might discuss, and (c) the manner in which the group is to be led; and (3) the suggested chairman of such a group. It is expected that such a discussion group will consist of no more than 30 persons.

Division 16. School Psychology. Groups doing research in fields related to school psychology are invited to submit proposals with suggested discussants.

VII. BUSINESS MEETINGS AND SPECIAL SESSIONS

The chairmen of APA boards and committees will receive in February inquiry forms for indicating their requirements during the convention.

Luncheons, dinners, business meetings, and social hours may be scheduled for both APA and for other groups. All requests must be sent to the Chairman of the APA Convention Program Committee. Inclusion in the program of such requested events on the part of non-APA groups will be at the discretion of the APA Convention Program Committee. If such events are to be considered for approval and announcement in the July *American Psychologist*, they must be received before **April 15** for APA groups and before **March 15** for non-APA groups. After these APA Convention Program Committee deadlines, requests are to be sent to the Associate Convention Manager.

VIII. FILM PRESENTATIONS

APA members, commercial film producers, or distributors who wish to present new films, film strips, or other audio-visual aids (including sound recordings) should send them insured and prepaid before **March 1** to the Chairman of the Film Presentations Committee (see Section XI). This committee will review and select the materials which will be presented as a part of the printed program. If the showing of more than one film or recording is requested, the order of preference should be indicated. All commercial film producers or distributors will be assessed a handling charge for accepted films.

IX. EXHIBITS

At the discretion of the APA Convention Manager, APA members may be granted free space for noncommercial exhibits of apparatus, teaching aids, and other materials of scientific and applied interest. Members must arrange for their exhibits before **April 15**. At the discretion of the APA Convention Program Committee, nonprofit journal publishers may also exhibit their journals free of charge provided requests are received before **April 15**. Commercial agencies are invited to request arrangements for exhibits; the deadline for these agencies is **July 15**. All commercial exhibitors will be charged for space. APA members and publishers of nonprofit journals should write to the Chairman of the APA Convention Program Committee indicating the type of exhibit and space needs. Commercial organizations wishing to arrange for exhibits should correspond with the Chairman of the Exhibits Committee.

X. SUMMARY OF DEADLINES

FOR	DATE
Symposia:	
In final form	March 15
In proposal stage	March 1
Papers	March 1
Suggestions for additional programs	March 1
Requests for business meetings, luncheons, dinner, and social hours:	
Non-APA groups	March 15
Divisions and Psi Chi	April 15
Film presentations	March 1
Exhibits:	
APA members	April 15
Nonprofit journal publishers	April 15
Commercial organizations	July 15

XI. APA CONVENTION CHAIRMEN

APA Convention Program Committee

Dr. Harold Guetzkow
1333 Sixteenth Street, N.W.
Washington 6, D. C.

Convention Manager

Mr. George S. Speer
Institute for Psychological Services
Illinois Institute of Technology
3329 South Federal Street
Chicago 16, Illinois

Associate Convention Manager

Dr. Philip Ash
Inland Steel Company
30 West Monroe Street
Chicago 3, Illinois

Exhibits Committee

Dr. Maurice O. Burke
Institute for Psychological Services
Illinois Institute of Technology
3329 South Federal Street
Chicago 16, Illinois

Film Presentations Committee

Dr. Paul Wendt
Southern Illinois University
Instructional Materials
Library 103-G
Carbondale, Illinois

XII. PROGRAM CHAIRMEN

APA Board of Professional Affairs

Mr. George S. Speer
Institute for Psychological Services
Illinois Institute of Technology
3329 South Federal Street
Chicago 16, Illinois

Division 1. Division of General Psychology

Professor N. H. Pronko
Department of Psychology
University of Wichita
Wichita 14, Kansas

Division 2. Division on the Teaching of Psychology

Dr. Ralph H. Turner
Department of Psychology
Oberlin College
Oberlin, Ohio

Division 3. Division of Experimental Psychology

Dr. Wilse B. Webb
Department of Psychology
University of Florida
Gainesville, Florida

Division 5. Division on Evaluation and Measurement

Dr. Donald W. Fiske
Department of Psychology
University of Chicago
Chicago 37, Illinois

Division 7. Division on Developmental Psychology

Dr. Daniel R. Miller
Department of Psychology
University of Michigan
Ann Arbor, Michigan

Division 8. Division of Personality and Social Psychology

Dr. Joseph B. Adelson
Department of Psychology
University of Michigan
Ann Arbor, Michigan

Division 9. Society for the Psychological Study of Social Issues

Dr. Martin L. Hoffman
Merrill-Palmer School
71 East Ferry Avenue
Detroit 2, Michigan

Division 10. Division on Esthetics

Dr. Wilson L. Taylor
1604 Forbes Street
Rockville, Maryland

Division 12. Division of Clinical Psychology

Dr. Nicholas Hobbs
Department of Psychology
George Peabody College for Teachers
Nashville 5, Tennessee

Division 13. Division of Consulting Psychology

Dr. Marie Paula Skodak
Personnel Counseling Service
530 East Third Street
Flint 3, Michigan

Division 14. Division of Industrial and Business Psychology

Dr. Joseph Weitz
Richardson, Bellows, Henry & Company
355 Lexington Avenue
New York, New York

Division 15. Division of Educational Psychology

Dr. Herbert Conrad
United States Office of Education
Washington 25, D. C.

Division 16. Division of School Psychologists

Dr. Elizabeth M. Drews
1410 Sherwood
East Lansing, Michigan

Division 17. Division of Counseling Psychology

Dr. C. H. Ruedisili
107 South Hall
University of Wisconsin
Madison 6, Wisconsin

Division 18. Division of Psychologists in Public Service

Dr. Lawrence S. Rogers
Veterans Administration Hospital
1055 Clermont Street
Denver 20, Colorado

Division 19. Division of Military Psychology

Dr. Walter F. Grether
425 Cherry Drive
Dayton 5, Ohio

Division 20. Division on Maturity and Old Age

Dr. Robert Watson Kleemeier
Department of Psychology
Washington University
St. Louis, Missouri

Division 21. Society of Engineering Psychologists

Dr. Ezra V. Saul
Institute for Applied Experimental Psychology
Tufts University
Medford 55, Massachusetts

Division 22. National Council on Psychological Aspects of Disability

Dr. C. H. Patterson
College of Education
University of Illinois
Urbana, Illinois

Psi Chi

Dr. Max Meenes
Department of Psychology
Howard University
Washington 1, D. C.

ADVANCED REGISTRATION FORM

SIXTY-EIGHTH ANNUAL CONVENTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

Chicago, Illinois, September 1-7, 1960

Please type or print:

Name: Dr.
Mr.
Mrs. Last First Middle Initial

Professional Affiliation:
 (to appear on badge,
 maximum of 20 letters
 per line)

(Street Address)
 (City) (State)

The following information is requested for the Convention Directory:

1. Expected date of arrival: _____ Departure: _____
2. APA membership status:
 Fellow _____ Member _____ Associate _____
 Member, Student Journal Group _____ Foreign Affiliate _____ Member, Psi Chi _____
3. Indicate Division memberships by number(s) _____
 There is no registration fee for APA Fellows, Members, Associates, Affiliates, and Members of the Student Journal Group or Psi Chi.
4. Nonmember _____

REGISTRATION FROM NONMEMBERS MUST BE ACCOMPANIED BY A REGISTRATION FEE OF \$3.00. MAKE CHECK PAYABLE TO: APA CONVENTION AFFAIRS BOARD.

The following information is requested to help plan special events and related facilities:

1. Is your spouse coming to the convention with you? Yes _____ No _____
2. Are you bringing any children with you? Yes _____ No _____ If yes, check below:
 a. Number in age group:
 Under 3 years _____ 3-6 years _____ 6-12 years _____ Over 12 years _____
 b. Do you want day nursery child care? Yes _____ No _____

APA DAY, SUNDAY, SEPTEMBER 4

..... I plan to attend the APA Day Buffet Lunch.
 Please reserve lunch ticket(s) for me at \$3.00 each, including tax and tip.
 Check for \$..... enclosed for lunches.
 I do not wish a reservation for lunch.

You may register either at the Sherman or Morrison. Duplicate registration facilities will be set up and maintained throughout the convention.

HOTEL INFORMATION

..... I do not want a hotel reservation

Headquarters will be the Sherman and Morrison Hotels. The following rates will apply in both hotels. Please indicate the hotel and type of accommodations desired:

..... Single bedrooms @ \$ 8.00 \$10.00 \$12.00
..... Double bedrooms @ 11.00 13.00 15.00
..... Twin bedrooms @ 14.00 15.00 16.00
..... Dormitory rooms @ 3.50 per person	(This rate applies only for at least four or more in a room. Please list names below.)	

Hotel desired: Sherman Morrison

NOTE: THESE RATES ARE GUARANTEED AT THE RATE REQUESTED IF THE REGISTRATION BLANK IS RETURNED PRIOR TO AUGUST 1, 1960. AFTER AUGUST 1 EVERY EFFORT WILL BE MADE TO ASSIGN ROOMS AT THE RATE REQUESTED, BUT SUCH ASSIGNMENT IS QUITE UNLIKELY, AND CANNOT BE GUARANTEED. IT IS MOST LIKELY THAT RESERVATIONS RECEIVED AFTER AUGUST 1 WILL BE ASSIGNED AT THE MAXIMUM RATE SHOWN HERE.

MEMBERS ARE URGED TO RETURN THIS FORM PROMPTLY, AND IN ANY EVENT PRIOR TO AUGUST 1, IN ORDER TO BE SURE OF RECEIVING THE ACCOMMODATIONS DESIRED.

Note: Reservations will not be held beyond 6:00 P.M. except by request. Your reservation will be confirmed.

Mail Confirmation to:
 Address: City: State:

Room occupants: (Be sure to give the names of all occupants.)

Name (Please print)	Sex	Address	City	State
.....
.....
.....
.....

Please send this form as early as possible to:

APA Housing Bureau, Room 900, 134 North La Salle Street, Chicago 2, Illinois

(Copies of this form may be obtained from the APA Housing Bureau or from the APA Central Office.)
 Advanced Registration Forms received after August 1 may not be processed for preregistration.

BOOKS, "SEPARATES," PAMPHLETS

Available at

THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

Books

- School Psychologists at Mid-Century.* 1955 (reprinted 1958). Pp. 230. \$2.75
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Order from:

AMERICAN PSYCHOLOGICAL ASSOCIATION

Dept. 1959

1333 Sixteenth Street, N.W.

Washington 6, D. C.

CALL FOR VOLUNTEERS

SIXTY-EIGHTH ANNUAL CONVENTION AMERICAN PSYCHOLOGICAL ASSOCIATION

Chicago, Illinois

September 1-7, 1960

It takes people—preferably people with psychological background—to run a convention. Would you be willing to help?

If you are planning to come to the convention and can devote at least one full day or two half days (9 A.M. to 1 P.M.; 1 P.M. to 5 P.M.) to one of the many jobs that must be filled to run a smooth convention, would you please indicate on the coupon the time and portions of the day that you would be available.

Please return to:

Dr. Benjamin Burack
Volunteer Workers, APA 1960
4880 North Marine Drive
Chicago 40, Illinois

I volunteer for an assignment at the APA Convention in Chicago in 1960 for the following day(s) and period(s):

	Wed. Aug. 31	Thurs. Sept. 1	Fri. Sept. 2	Sat. Sept. 3	Sun. Sept. 4	Mon. Sept. 5	Tues. Sept. 6	Wed. Sept. 7
9 A.M.-1 P.M.*								
1 P.M.-5 P.M.								

* Note: There *will be* lunch breaks.

Can you (check if YES): type.....; take shorthand.....

Name.....

Mailing Address.....

Contemporary Psychology

A JOURNAL OF REVIEWS

July 1959

VOLUME IV • NUMBER 7

Page 193	Words and Things , by Roger Brown <i>Reviewed by J. J. JENKINS</i>
196	Existence: A New Dimension in Psychiatry and Psychology , by Rollo May, Ernest Angel, and H. F. Ellenberger (Eds.) <i>Reviewed by C. R. ROGERS</i>
198	The Study of Leadership , by C. G. Browne and T. Cohn (Eds.) <i>Reviewed by G. C. HOMANS</i>
199	Leader Attitudes and Group Effectiveness . Fiedler <i>Reviewed by E. A. FLEISHMAN</i>
200	The Unsilent Generation , by Otto Br. <i>Reviewed by W. L. BARNETTE, JR.</i>
201	Young Man Luther: A Study in Personality History , by E. H. Erikson <i>Reviewed by H. G. McCURDY</i>
202	Aggression , by J. P. Scott <i>Reviewed by LEONARD</i>
204	CP SPEAKS By GARDNER M.
206	Common Sense in Teaching By NICOLAI

AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.
1333 Sixteenth Street, N. W., Washington 6, D. C.

Please send a sample copy of CONTEMPORARY PSYCHOLOGY to:

Name

Address

PUBLICATIONS

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480 pp. · Pub. 1959
Text price \$6.95

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by **HENRY BOROW**, Uni-
versity of Minnesota and
ROBERT V. LINDSEY,
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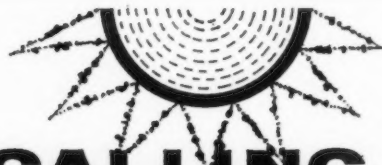
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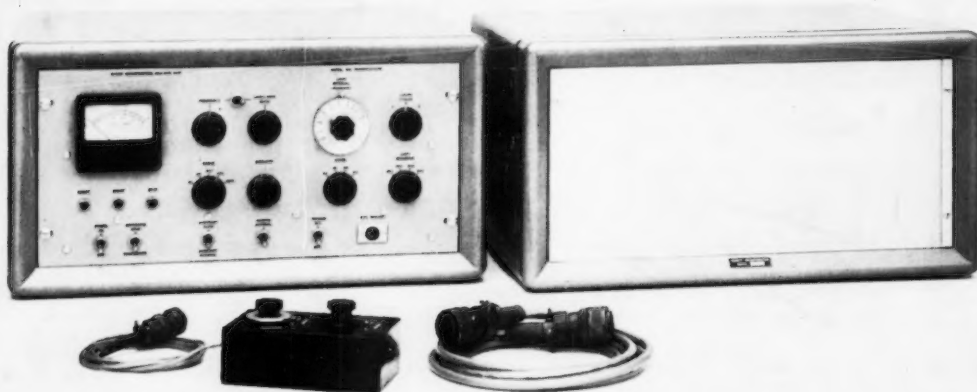
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